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PHILOSOPHICAL
TRANSACTIONS,
OF THE
ROYAL SOCIETY
OF
L O N D O N.

VOL. LXXIV. For the Year 1784.

P A R T I.

L O N D O N,

SOLD BY LOCKYER DAVIS, AND PETER ELMSLY,
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LABORATIONS
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ALABAMA

1897



ADVERTISEMENT

THE Committee appointed by the Royal Society to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations, which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great

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per for publication in the future *Transactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

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It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the chair, to be given to the authors of such papers, as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports, and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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THE President and Council of the Royal Society adjudged, for the last Year, 1783, Two Medals on Sir GODFREY COPLEY's Donation; One to JOHN GOODRICKE, Esq. for his Discovery of the Period of the Variation of Light in the Star Algol; and the other to THOMAS HUTCHINS, Esq. for his Experiments to ascertain the Point of Mercurial Congelation.

E R R A T A.

Page. Line.

- 117. 7. *for canon read cannon*
- 164. 9. *for not read nor*
- 181. 8. *for called read call*
- 211. 7. *for Tweed read Clyde*
- 224. 4. *for of read or*

PHILOSOPHICAL
TRANSACTIONS

- I. *An Observation of the Variation of Light in the Star Algol.*
In a Letter from Sir Henry C. Englefield, Bart. F. R. S.
and S. A. to Joseph Planta, Esq. Sec. R. S.

Read November 6, 1783.

S I R,

July 3, 1783.

HAVING been fortunate enough, from the fineness of the last night, to make a satisfactory observation of the variation of Algol, I lose no time in communicating it to the Society.

The last visible period was June the 10th, when Mr. AUBERT, as well as myself, observed it, though imperfectly,
VOL. LXXIV. B and

and thought the time of its greatest diminution was about $2\frac{1}{2}$ h. in the morning; calculating from thence by Mr. GOODRICKE's period of 2 d. 20 h. 48', the time of least brightness was to be about one o'clock this morning.

All the following observations were made with an excellent night-glass, magnifying about eight times, with a field of 5° ; in which therefore Algol and the ρ were distinctly visible at once.

I first looked out at midnight, and readily found the star, though hardly visible to the naked eye from the vapours near the horizon. It appeared much bigger than the ρ , and full as big again as the π , also in the field at the same time.

At $12\frac{1}{2}$ h. I looked again, and saw but little difference, as Algol was then also evidently much brighter than ρ . I at that time faintly perceived it with the naked eye.

At 1 h. 10' the star was but very little bigger than ρ , the diminution having gone on most rapidly in the interval between the two last observations. Though higher above the horizon it was much less (if at all) visible to the naked eye.

At 1 h. 35' it was, I think, diminished (though but little) since the former observation. It was still, however, a very little larger than ρ , but not at all visible to the naked eye.

At 2 h. it was scarce at all altered from the last observation; but, if any thing, seemed recovering its light.

I had meant to observe its progress still further; but returning to the glass at half an hour after two, clouds had suddenly covered the whole sky.

The fact of the diminution of Algol is, however, fully confirmed (if confirmation was wanting) by this observation, and the accuracy of the period fixed by Mr. GOODRICKE ascertained,

tained, as the phænomenon was certainly within half an hour of the time fixed by Mr. GOODRICK, which, divided on eight periods, gives only an error of four minutes on the length of it; and a nearer coincidence is not to be expected in a matter of this nature, where estimation is the only means of determining the brightness, and two persons can hardly agree within a few minutes, from the difference of sight.

I am, &c.

II. *Observations on the Obscuration of the Star Algol, by Palitch, a Farmer. Communicated in a Letter from the Count de Bruhl, F. R. S. to Sir Joseph Banks, Bart. P. R. S.*

Read November 13, 1783.

S I R,

Nov. 7, 1783.
Dover-Street,

I AVAIL myself of the permission you gave me, when I had the honour to meet you yesterday in the Drawing-room, by sending you the following short account, which was transmitted to me by Mr. CANZLER, one of the Elector's Librarians, dated Dresden the 10th of October. PALITCH, a farmer of Prolitz, a village in the neighbourhood of that Residence, saw the greatest obscuration of Algol on the 12th of September, at eight o'clock, P. M. On the 2d and 5th of October he observed the same phenomenon again. On the 5th the greatest diminution of that star's light happened some minutes before seven, when he judged it nearly of the size of a star of the fourth magnitude: it continued increasing in brightness till a quarter past ten in the evening, at which time it had entirely recovered its usual brilliancy and size. From his own observations he estimates the period of that remarkable phenomenon at 2 days 20 hours 53 minutes.

I have the honour to be, &c.



III. *Further Observations upon Algol. By the same.*

Read January 15, 1784.

OCT. 20th, PALITCH saw Algol nearly at its greatest obscuration, at 3 o'clock in the morning.

OCT. 22d, near 12 P. M. he observed it again in the same state.

OCT. 25th, at about 9 P. M. it appeared to him like a star of the third magnitude. He was prevented by clouds from making long observations; but as all those he has had opportunities to make, indicate a period somewhat longer than that of 2 days 20 h. 51' he is inclined to think that half the difference between that period and his own, *viz.* 2 d. 20 h. 52' will come very near the truth.

IV. Descriptions of the King's Wells at Sheernefs, Languard-Fort, and Harwich. By Sir Thomas Hyde Page, Knt. F. R. S.; communicated by Licut. Gen. Rainsford, F. R. S.

Read November 13, 1783.

LIEUT. GEN. RAINSFORD.

S I R,

St. Margaret's-Street,
March 28, 1783.

I HAD the pleasure to receive your obliging letter of the 28th ult. mentioning, that it would be satisfactory to the Royal Society to have a description of the wells at Sheernefs, Harwich, and Languard-Fort, which were made under my direction, whilst I commanded as Engineer at those places.

I beg to acquaint you, that it will be necessary to mention some previous circumstances that occasioned those undertakings, which will rather interfere with the descriptive part, and I fear intrude on the patience of the Society; but I shall in this respect hope for their indulgence, it being my wish to explain the nature of the different operations as fully as possible, that similar situations, where water is wanted, may receive benefit from the experiments I have had the good fortune to succeed in; and it cannot fail of affording me the highest satisfaction to have an opportunity of communicating this subject to the knowledge of the public through the Royal Society.

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I have only further to request, that you will do me the honour to lay the following descriptions before Sir JOSEPH BANKS and the Society; and as you are fully acquainted with the subject, you will confer on me an additional favour by explaining the several parts (if requisite) more fully than I have done in the written account, when it is under consideration.

I am, &c.

Some circumstances respecting the garrisons of Sheerness, Landguard-Fort, and the Town of Harwich, with a description of the wells which supply water for the use of the troops, &c. at each place.

The Master-general of the Ordnance (Lord TOWNSHEND), in the beginning of the year 1778, recommended to his Majesty, that the fortifications upon the Eastern Coast, including Dover, Sheerness, Landguard-Fort, and some other places, should be repaired, and new works added, where they might appear necessary towards a proper state of defence, if a war with Holland, or other Northern powers, was found unavoidable. His lordship foresaw the great objection to fortifications, in the want of fresh water under the command of the guns of our garrisons; and I had directions accordingly to consider the subject, and report to his lordship and the Board of Ordnance any ideas that might be likely to remedy so great a defect.

The dock-yard and garrison at Sheerness were supplied with water from Chatham at an enormous expence, near two thousand

land pounds *per annum*, or occasionally from Queenborough, neither of which supplies could be continued in case of a siege, which of course would be of short duration from this circumstance. Some attempts had been made in former times to obtain water on the spot, by sinking wells, but they had failed; and success in such undertakings was at last considered as impossible, from the great difficulty they had met with in the vast quantities of sea-water, that came by filtration through the sands into their wells, and rendered a progress to any considerable depth impracticable. It is probable, that the course of the river Medway has undergone many changes, and had once an out-fall to the sea, near the high ground of the Isle of Shepey. The docks, garrisons, buildings, &c. for a considerable distance into the island, consequently stand upon very loose materials, which were found, upon sinking the well in Fort Townshend, to consist of mud, sea-beach, and quicksand, nearly to the present depth of the river Medway, and admit so strong a filtration of salt-water, as must ever render the sinking of wells exceedingly difficult. This was the situation in which I found Sheerness.

Landguard-Fort was not more eligible respecting water, as a place of strength. It was, indeed, better supplied under any other consideration, a pipe being laid into the place from a good spring about two miles distant, which furnished a plentiful quantity of water; but such is the disadvantage of situation that, in case of attack, that spring must fall into the possession of the enemy, and our garrison of course would be deprived of its use. This was a serious consideration and objection to a great extent of fortification, however eligible in other respects the place might be.

Harwich was judged by the Commander in Chief (Lord AMHERST) to be a very proper station for a considerable part of the army, in time of war with Holland, as central to furnish detachments for such parts of the coast as might be in danger, as also to cover a very useful harbour and increasing dock-yard; but his lordship was sensible of the want of wholesome water in that neighbourhood, and gave particular directions to establish such a supply for the camp to be formed there, as might appear proper for the health of the troops; and the subsequent orders given by General RAINSFORD, who commanded that district, perfectly answered every desirable end, until good water was found within his camp. The inhabitants of the town of Harwich had chiefly depended on rains for their supply, the wells being in general brackish from the filtration of salt-water. The neighbourhood, to many miles distance, was not better furnished, there being only stagnating water in ponds or shallow wells, which were supplied from the upper surface of the ground; and, whether rendered bad by a mixture of copperas, or other mineral, it was not such as could be given for the use of the troops with any degree of prudence or attention to their health, and they were, to avoid dangerous consequences, furnished with water, by General RAINSFORD's order, from the opposite side of the Manningtree River, by boats employed for that purpose, the beginning of the first campaign.

I will now endeavour to describe the experiments at each place, beginning with the well in Fort Townshend at Sheerness, which with a reference to the plans will, I hope, render the subject sufficiently intelligible.

King's Well, Fort Townshend, Sheerness.

This undertaking was at first considered as a mere experiment, the probability of success being much against it; I however thought the attempt, where a dock-yard of great consequence to the navy was established, should be made, and carried as far as it could, with a proper attention to œconomy in laying out the money of the public. Such was my opinion signified to the Master-general and the Board of Ordnance. I received an answer thereto, expressive of approbation, and full powers to employ proper persons, and proceed upon the undertaking.

These previous steps seemed highly necessary, as in all works of difficulty, great confidence is as much required as able workmen or good plans.

The favourable opinion his Majesty was graciously pleased to express publicly of the project, when he visited Sheerness, and saw the well, tended very much towards its final success; and the countenance and support of General CRAIG, governor of that garrison, greatly promoted perseverance in a work of such difficulty.

I employed a very ingenious man, Mr. COLE, engine-maker, of Lambeth, as a chief person in this business, and received every assistance I expected from his experience and judgement in mechanics; and it is but justice to him to express, that the success of the work greatly depended on his attention and the able assistants he procured from distant parts of the kingdom.

The greatest acknowledgement is also due to the ability of Lieut. HUMFRYS, of the Engineers, and Mr. MARSHALL, the Ordnance-overseer, who were constantly on the spot, and car-

ried my orders into execution with the greatest zeal for the success of the undertaking as well as judgement. The mentioning these gentlemen's names is, as well as a justice to their conduct, to recommend harmony and mutual exertion in any future work of this nature, as, without an equal attention in every one, I should greatly doubt success, even admitting the same plan to be in all other respects strictly attended to, as there would be great difficulty and danger to the lives of the workmen if carelessly carried on.

The work was begun the 4th of June, 1781, and finished the 4th of July, 1782.

A circle of twenty-two feet diameter was first marked out on the ground, and the space excavated to the depth of five feet; after which, pieces of wood, called ribs, upon the curve of a diameter twenty-one feet four inches, and about nine inches scantling, were placed, to form a complete circle within the excavated part at the bottom, above which other circles of the same nature were placed, and supported by upright pieces of scantlings, having short boards introduced by the intervals, which afterwards pressed upon the circles or ribs, between them and the exterior parts. These, when united, formed one frame of wood from the bottom to the top, or rather higher than the excavated space, and prevented the mud of the upper surface, which was very soft, from falling in upon the workmen. In proceeding deeper, care was taken to prevent the sinking of the before-mentioned frame by its own weight, in excavating parts only under it till another circle of pieces like the first, called ribs, was formed, and uprights, with boards behind, introduced. The distance between these circles was in the first, or upper part of the work, about three feet; but as difficulties increased they were placed nearer, and

in many parts joined each other without any boards or uprights (as will appear in the section of the plan), and continued through the whole of the wooden frame, against the weight of the mud, quick-sand, and sea-beach, to the depth of thirty-six feet.

The reason of the circular frames being nearer in some parts than in others, arose from the greater or less quantity of salt-water that came through the sands, &c. and often rendered it impossible to sink under the frame more than the thickness of one of the ribs, without danger of blowing-up, or of the sides behind the wood slipping with the streams of water, and thereby forcing into the bottom of the well, which in sinking through very wet quick-sand is much to be apprehended; and an accident of that nature would entirely destroy the work. An attention to the plan will shew at what depths the filtration of water was most dangerous, and the difficulties at different periods, may be estimated by the distance of the circles, formed of ribs, from each other, and where they appear to join, it was not without the utmost efforts of labour that the work could be carried on. At the depth of thirty-six feet the wood-work was finished, and six feet deeper a firm foundation of hard blue clay discovered. The several parts of the frame were then strengthened wherever it appeared necessary, to prevent separation, and to resist the immense pressure of soft mud, quick-sand, and loose sea-beach, which were supported by it.

It must be observed, that the salt-water, after proceeding thus far, came in very fast through all the joints of the frame, and that holes were left on purpose in certain parts to let it run into the well, that it might not be confined entirely to the bottom of the work, which, from the weight upon one part
only,

only, might have blown, which is ever (as has been observed) to be guarded against with the utmost caution.

The frame being found of sufficient strength, and the workmen able, by constant drawing with four 36-gallon buckets, to keep the bottom of the well dry enough to proceed further, the greatest difficulty seemed to be overcome. The next process was to cut off or stop the salt-water out entirely: to effect which, a smaller circle was described at the bottom of the well, upon the hard clay already mentioned, of the diameter of eight feet in the clear, round which a curb, or circular frame of wood, was laid, and a brick steening, of two bricks thick in tarris, raised gradually towards the top of the well, whilst, as it proceeded upwards, the space between the back of this steening and the wooden frame (fixed six feet higher) was filled with good tempered clay, four feet thick, and carefully rammed. During this operation, and raising the brick-work, with the clay behind it, the water continued to run over them into the center of the well, now reduced to eight feet diameter, and was constantly drawn out, to leave the workmen on the sides sufficiently dry to raise their work until they had reached the top, and consequently, as it was water-tight, cut off the filtration from the sea, precautions having been taken to prevent the danger of blowing at the bottom.

The next proceeding appeared more simple; but great care was still necessary to avoid damaging the foundation of the works already done, as the least crack might have again introduced the salt-water. A smaller circle than the last was therefore described, and ribs, forming circles of wood, raised some feet within the brick-work; and others, of the same form, were sunk to the depth of eight feet below the bottom, upon which the several works already described rested. After this a
course

course of bricks was carried up within the last mentioned ribs or circles, upon a diameter of six feet, whereby they became inclosed and joined with the first mentioned brick-work, having the clay wall and wooden frame pressing behind them upon larger diameters. In sinking lower, small curbs were at certain distances (as will appear in the section of the plan) placed to support the steening, which consisted of two stretching courses of bricks, laid separately, and keyed into the clay or back part of the brick-work by rough pieces of stone, flint, &c. to prevent a slipping or lowering of the steening by its own weight. The work was carried on from this period, without any material difficulty or difference in the clay (except the very extraordinary discovery of a piece of a tree at the depth of 300 feet from the top of the well, which is shewn in the plan) until the appearance of water at 328 feet deep, by a small mixture of sand in the clay, with oozing of water from it; and at 330 feet deep, upon boring, the whole bottom of the well blew up, and it was with difficulty the workmen escaped the torrents of water that followed them, which was mixed with a quick-sand that rose forty feet in the bottom of the well, at which height it still remains. The water rose in six hours 189 feet, and in a few days within eight feet of the top of the well. It has since been carefully analyzed by a chemist, and found perfectly good for every purpose; and, it is presumed, the quantity will be equal to every demand of public and private use at that place, as there has been, ever since it was first discovered, a constant drawing of water, and it has hitherto been found impossible to lower the well more than 200 feet, there has consequently always been a depth left in water of 130 feet. It is to be remarked, that the water is of a very soft quality, and, upon being drawn, has a degree of warmth unusual in common

common well-water. It remains yet to be determined whence that warmth proceeds; but as it proved wholesome, the circumstance is fortunate for the soldiers of the garrison, as they will not be liable to complaints that are so frequent among troops (as often happens at Dover Castle) from imprudence in drinking great quantities of very cold well-water.

King's Wells at Landguard-Fort.

They were begun and finished in the year 1782.

The peculiar situation of this fort made it very unlikely that springs of fresh-water could ever be found, there being great reason to think, that the out-fall of the Ipswich and Manning-tree Rivers, which unite before they reach the sea, was formerly on the Suffolk side of the fort, but is now on the Essex side; and as the garrison, in ancient writings, is described to have been built on the Andrew's Sand, there appeared little probability of any filtration of water through it, except that of the sea. It, however, seemed proper to try the possibility of sinking through it, to endeavour to find a hard bottom, similar to that discovered at Sheernefs, fresh-water being of vast consequence to the defence of the place. The work was accordingly begun; but about the same time, in making the excavation of a ditch for one of the batteries, at a very few feet from the upper surface of the sand, a small quantity of fresh-water was perceived; and it was chance that led to a discovery of its freshness, from one of the labourers happening to taste it. The circumstance

circumstance was reported to me by Mr. ROBERTS, the Adjutant of the Works; and we, upon examining further, found that the quantity of water upon sinking was considerable, and that it appeared perfectly fresh. I then ordered the well-sinkers to proceed to this depth at another place, where they found a like appearance of good water; and the quantity was so great, as to render it very difficult to keep the bottom of the well, at twelve feet deep, dry enough to sink further. Every exertion was notwithstanding used, and with great labour a well was sunk to the depth of low water mark at spring tides, about eighteen feet from the upper surface of the sand; when, to the surprize of every person, the water that rose from the bottom became, on a sudden, entirely salt. This put an end to the work for a time, as it seemed impossible to penetrate deeper. I then considered the matter very differently with my first idea, and though the impossibility of having a deep well clearly appeared, there remained a prospect of a sufficient supply of good fresh water. It may now be necessary to recollect, that at a very few feet from the surface (eight feet) there was good water; that it continued in vast quantity almost to the spring tide low-water-mark, after which the salt-water had appeared; I therefore directed sand to be thrown into the well, to bring it a little above what had been the *lowest fresh-water* line (twelve feet from the upper surface) and then drew the water out which had mixed. After this, the filtration into the well became again perfectly fresh, and in equal quantity to the first appearance. This was, therefore, fixed as the greatest depth (twelve feet) and another well sunk at forty feet distance, with a horizontal brick drain, having holes left in the sides for filtration, as described in the plan, to collect the water, and the bottoms of both wells were secured with hard materials; that the whole supply

supply of water might be reduced to the drain, which is constructed to prevent as much as possible the mixture of sand with the water, and is found to answer the desired end. This success arose from various unexpected circumstances; but I am yet at a loss for the cause of the fresh water, or whence it comes.

I conceive, that there is a certain distance from the sea, upon every sandy shore, to which the salt-water penetrates, where it is forced whilst the tide is at its greatest height; and that such water, when so far pressed into the sands, has an action back towards the sea again, as *the tide falls*, and continues to have it until another tide makes it revert; this may account for the filtration of salt-water a certain way into a country; and that further, from *probably higher surfaces*, there may be fresh-water in the same continuation of sands, and the separation discoverable to a degree of great accuracy; whether this action of salt-water in the sand, by friction, can render it fresh, or of a less degree of salt, I will not pretend to judge. I presume the contrary; but am even under that idea at a loss to know how so much fresh water gets into the sand at Landguard-Fort, it being so entirely separated from the spring of the country. It is evident, upon a full consideration of the subject, that the sea, to the height of *low water*, will penetrate a vast distance into a sandy country, by filtration, and to that height *only*, it having so far a constant pressure, and no re-action; the water, therefore, being once in the sand, can never return by the same passage, the cause of its entrance still remaining; whereas in the higher surfaces, the *rise* and *fall* of tides must keep it in constant movement, and the distance of filtration will bear a proportion to the duration of pressure which gave it original motion. It is probably not so easy to account for a body of

fresh-water being to the depth of twelve feet in the sand, and in the same line, a few feet deeper, the water should be entirely salt, and that they do not mix together. Whether the greater specific gravity of the salt-water is sufficient to prevent a mixture with the fresh upon a higher line, I cannot venture to say; but the fact of there being a separation is beyond a doubt, and the depths may be ascertained to a degree of great accuracy. However this may be accounted for, the discovery at Landguard-Fort is of very great consequence to the garrison; and there is reason to think, that in similar situations, where water is wanted, an attention to what has been already explained may be found of use.

King's Wells at Harwich.

They were begun the 6th of May, 1781, upon General RAINSFORD's taking the command at that camp, and finished the 19th of September following.

The wells in this neighbourhood, as has already been observed, being very shallow, and only depending on springs from the upper surfaces of the ground, have but little water in the summer, and the quality of it is very bad. The best of the old wells was in the rear of General RAINSFORD's camp, and was thought of at first for the use of the troops; but he prudently declined that supply. It was imagined, as the water from the upper surface was of a bad quality, that the most likely way to obtain a better spring was to sink a well from higher ground, and to endeavour to penetrate through a rock which lay a few yards under the level of the country, although the operation might be tedious, upon the chance of cutting a spring

spring of better water, that might be unconnected with the land-drains. The experiment answered in every respect, as there was not a drop of water found till the rock had been entirely cut through, when, upon finding a considerable quantity of moist sand, and boring into it, a plentiful spring was discovered, and has supplied the troops ever since with very good water. It is probable this supply, the spring being very powerful, will be found equal to every demand for public and private purposes, in the driest seasons. After this success, as matter of curiosity, an old well was made deeper, by excavating through the rocks, where a good spring was also found; but as that well had been originally sunk from low ground, a great deal of the bad water from the upper drains, &c. mixes with it, and gives it a disagreeable taste.

The plans will describe the manner of making these wells sufficiently. I have chiefly dwelt on the descriptive part, to recommend, where it is apprehended any mineral or drain from the upper surface of lands, by mixing in wells, may hurt the water, the sinking from the heights, as there are few countries where very good water may not be found, by a proper attention to locality in making wells.

EXPLANATION OF THE PLATES.

Tab. I. fig. 1. Section of the King's Well in Fort Townshend at Sheerness.

2. Plan of the frame and well.
3. Section of the frame AA.
4. Plan of the well.

Tab. II. X. Line of high-water mark.

Y. Line of low-water mark.

Z. Line of low-water at spring-tides.

V. Extract of a Letter from Edward Pigott, Esq. to M. de Magellan, F. R. S.; containing the Discovery of a Comet.

Read November 27, 1783.

S I R,

York, Nov. 22, 1783.

I HAVE the pleasure of informing you, that I discovered a comet on the 19th instant, and have made the following observations on it.

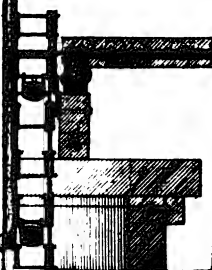
Date.				R. A.		North Decl.	
1783	h.						
Nov 19	11	15	-	41	0	-	3 10
20	10	54	-	40	0	-	4 32

Nov. 21. This night I saw the comet where I expected ~~it~~ according to the above determinations; but could not observe it with an instrument.

The comet looks like a nebula, with a diameter of about *two minutes* of a degree. The nucleus being very faint, is seen with some difficulty, when the wires of the instrument are illuminated. It is not visible with an opera glass.



Sheer



VI. *Project for a new Division of the Quadrant.* By Charles Hutton, LL.D. F. R. S. In a Letter to the Rev. Dr. Maskelyne, F. R. S. and Astronomer Royal.

Read November 27, 1783.

DEAR SIR,

Royal Mil. Acad. Woolwich,
Aug. 12, 1782.

HAVING long since thought it would be a meritorious and useful service, to adapt the tables of sines, tangents, and secants, to equal parts of the radius instead of to those of the quadrant; and having frequently mentioned this project to you, SIR, as a proper judge and promoter of all useful improvements in science; I now beg leave to lay before you some observations I have thrown together on the subject, with a view to stimulate others, either to undertake and calculate some part of so large and painful a work, or to communicate farther hints for the improvement and easier performance of it.

I have the honour to be, &c.

A project.

A project for constructing fines, tangents, secants, &c. to equal parts of the radius.

I. The arbitrary division of the quadrant of the circle into equal parts by 60ths, which has been delivered down to us from the ancients, and gradually extended by similar sub-divisions by the moderns, among various uses, serves for trigonometrical and other mathematical operations; by adapting to those divisions of the arc, certain lines expressed in equal parts of the radius, as chords, fines, tangents, &c. But among all the improvements in this useful branch of science, I have long wished to see a set of tables of fines, tangents, secants, &c. constructed to the arcs of the quadrant as divided into the like equal parts of the radius as those lines themselves. In this natural way, the arcs would not be expressed by divisions of 60ths, in degrees, minutes, &c., but by the common decimal scale of numbers; and the real lengths of the arcs, expressed in such common numbers, would then stand opposite their respective fines, tangents, &c. The uses of such an alteration would be many and great, and are too obvious and important to need pointing out or enforcing. I have therefore had for a long time a great desire to commence this arduous task; but continual interruptions have hitherto prevented me from making any considerable progress in so desirable an undertaking. But I am not without hopes that some future occasion may prove more propitious to my ardent wishes. It is not, however, to be expected, that this work can be accomplished by the labours of one person only; it will require rather the united endeavours of many. I shall therefore explain a few particulars relative to my project of this work, with a view to obtain from others, who

who may have leisure and abilities for it, their kind assistance, either by communicating hints of improvements, or by undertaking some part of the computations, to which they may be excited by their zeal for the accomplishment of so important a work, and by the extreme facility with which the calculations in this way are made.

2. In the first place then I would observe, that I think it will be sufficient to print the sines, tangents, &c. to seven places of figures; and that therefore it will be necessary to compute them to ten places, in order effectually to secure the truth of the seventh place to the nearest unit.

3. I would assume the radius equal to 100000, or suppose it to be divided into 100000 equal parts. Then it is well known, that the semi-circumference will be 314159.26536 nearly, and consequently the quadrant nearly 157079.63268 of the same equal parts, which is less than 157080 by .36732, or nearly $\frac{1}{3}$ of an unit, or nearer $\frac{3}{8} = .375$, or nearer $\frac{4}{11} = .3636$, or still nearer $\frac{7}{19} = .3684$, or still nearer $\frac{11}{30} = .36666$ &c. And the half quadrant, or $\frac{1}{2}$ of the circle, 78539.81634 which is less than 78540 by only .18366, or nearly $\frac{1}{5}$ only of any of the above-mentioned fractions.

4. The table may consist of five or more columns; the first column to contain the regular arithmetical series of arcs differing by unity, from the beginning, in this manner, 1, 2, 3, 4, 5, &c. up to half the quadrant, the next less whole number being 78539; then for the higher numbers, or those in the latter half quadrant, besides adding 1 continually, there must be at the first added the decimal .63268, which will make all the numbers in this half become the exact complements of the first half, which consists of whole numbers only; and these will be the lengths of the arcs. Or, in order to include the

quadrantal

we must also add 1'' more at every 16, which will make 3'' to be added at every 16th time, and 2'' at every other time besides; but the first time the 3'' must be added will be at the arc or number 8, to have them to the nearest second, the repetition of the fraction at the arc 8 amounting to above $\frac{1}{4}$ a second; and then the 3'' must be added at every 16 afterwards, *viz.* at 24, 40, 56, 72, 88, 104, &c.

10. But besides the constant addition of 2'' every time, and of 1'' more every 16th time, there must be 1'' more added for every $6753\frac{1}{2}$ time, on account of the excess of the fraction $\cdot 062648062470964$ over the fraction $\cdot 0625$ or $\frac{1}{16}$: for that excess is $\cdot 000148062470964$ which is $= \frac{1}{6753\frac{1}{2}}$. And the easiest method of making this last addition of 1 at every $6753\frac{1}{2}$, will be to make the increase of the 1 on account of the $\frac{1}{16}$ at an unit sooner for every $422\frac{3}{4}$; because 16 is $422\frac{3}{4}$ times contained in $6753\frac{1}{2}$; by which means the incremental units for the $\frac{1}{16}$ will become 1 more at that number $6753\frac{1}{2}$, which last unit may be considered as the increment of the former increment for the $\frac{1}{16}$, and so proceed up to the quadrant; which will complete the second column of arcs to the nearest second in each number. Or this second column may be exactly computed to as many decimals as we please, by adding continually the 2'' and decimals, *viz.* $\cdot 062648062470964$. But at the middle of the quadrant, where the numbers return again upwards by the right-hand, there will for once be to be added only the seconds and decimals answering to the arc $\cdot 63168$, *viz.* $1\cdot 30499618$ seconds, that number being necessary to make the numbers on the right-hand to be the exact complements of those on the left. Or it will, perhaps, be proper to make them to the nearest unit in the 6th place of decimals. And to fill

fill up the second column to this degree of accuracy, add continually 2.062648 seconds, but at the 9th line add 1 more, or 2.062649, because 9 times 062470964 amounts to 56223867, or more than half a unit at that place; and after that add 1 more than 2.062648 at every 16th line, viz. at 25, 41, 57, 73, &c. because 16 times 062470964 amounts to .99953542, or nearly 1, it being only .00046458 less than 1. And this number .00046458, thus added too much, will, in 134 times adding it, amount to more than 06223867, the excess of 56223867 above 5, or half a unit, at that place; therefore at the line or number 2153 (or $9 + 16 \times 134$) which would be to have the 1 more added, let the 1 be there omitted, and add it at the next line or 2154, the true decimals after the first six, for 2153 being 499985, and for 2154 they are 562456. Continue thus always adding 1 more at every 16th line, except at the following numbers, where the 1 must be omitted, and added at the next following number; viz.

2153	10765	19377	27989	36601	45213	53825	62437	71033
4314	12926	21538	30150	38746	47358	55970	64582	73194
6459	15071	23683	32295	40907	49519	58131	66743	75339
8620	17232	25844	34456	43052	51664	60276	68888	77500

And thus proceed to the middle of the quadrant; by which means all the numbers will be to the nearest unit in the sixth or last place. Also, to have a check upon these numbers at certain intervals, it may be proper to proceed in this manner: First find every 100th number, by adding its decimal .264806 &c. verifying them at every 10th; then find every 16th number, by adding continually .002369 &c. which will also be checked and verified at every 25th addition by one of the former set of 100, for 25 times 16 make 400, using a proper pre-

caution to preserve each number true to the nearest unit in the 6th or last decimal.

As to the decimals of the numbers in the latter half of the quadrant, they will be the complements, to 1, of the corresponding numbers in the first half; and therefore they may be all easily found by taking each figure from 9, and the last from 10. But it will be safest to find only every 10th decimal in this way, and to fill up the intermediate nine by adding, as before, the constant decimal 062648; by which means they will be checked and verified at every 10th number.

11. To fill up the third column, or that of sines, as well as those of tangents and secants, it may first be observed, that the old tables of those lines to every minute, or even to every ten seconds of the quadrant, cannot be of so much use as it might seem at first sight; as the very near coincidence of the numbers in the new and old divisions appear very seldom to happen. I find, indeed, that our arc 1309 answers nearly to 45 minutes, that arc exceeding 45' by only .00632363 or $\frac{1}{158}$ part of a second nearly, and so in proportion for their equi-multiples. But although this degree of coincidence may be sufficient for checking the corresponding values of the arcs in the first and second columns, we are not thereby authorized to consider the sine, tangent, or secant of 1309 as accurately equal to that of 45' in all the seven places of figures, but differing from it by nearly the $\frac{1}{158}$ part of the difference corresponding to 1'', which is about $\frac{1}{4}$ of an unit in the sines and tangents, though next to nothing in the secants. This, therefore, although it makes no sensible difference in this particular case, will cause a difference that must not be neglected in the equi-multiples of 1309 and 45', the sines and tangents of which will differ by half a unit or

3
more,

more, and therefore will not be expressed by the same number, but will have some small difference in the seventh or last figure. And the same will happen in almost all the other arcs; so that generally the sines, &c. which are exact for the arcs in the first column, will not be quite so for those in the second, when expressed in whole seconds only, since these will sometimes differ by the part corresponding to almost half a second. However, in this, or any other case, where the difference is exactly known, we may profitably make use of the numbers in the old tables for constructing or verifying those of the new, by taking in the proportional part of the difference. Let, therefore, all the sines, &c. of every 1309 be computed from the old tables, and entered in the new, by adding to the sine, &c. of the corresponding multiple of 45' the like multiple of the $\frac{1}{1309}$ part of the proportional difference for 1''. This will give about 120 sines, &c. to serve as a verification of the computations by the more general methods. But if the second column be exactly constructed with all its decimal places by the continual addition of 2.06264807, the old tables may be converted into the new, by allowing for the odd seconds and decimals. And for this purpose it will, perhaps, be best to use the large table of RHETICUS, which contains the sines, tangents, and seconds, to ten places of figures for every 10'', and also the differences. At least, such sines, &c. may be found in this way as have their seconds and decimals well adapted for the purpose; and for such as would be found too troublesome in this way, recourse may be had to some of the following methods:

12. Let us now examine the expressions for the sines, &c. by infinite series.

The

The radius being 1, and arc a , it is well known that the

$$\begin{aligned} \text{fine is} &= a - \frac{1}{6}a^3 + \frac{1}{120}a^5 - \frac{1}{5040}a^7 + \frac{1}{362880}a^9 - \frac{1}{39916800}a^{11} \&c. \\ \text{cofine} &= 1 - \frac{1}{2}a^2 + \frac{1}{24}a^4 - \frac{1}{720}a^6 + \frac{1}{40320}a^8 - \frac{1}{3628800}a^{10} \&c. \\ \text{tang.} &= a + \frac{1}{3}a^3 + \frac{2}{15}a^5 + \frac{17}{315}a^7 + \frac{62}{2835}a^9 + \frac{1382}{155925}a^{11} \&c. \\ \text{cotang.} &= a^{-1} - \frac{1}{3}a - \frac{1}{45}a^3 - \frac{2}{945}a^5 - \frac{1}{4725}a^7 - \frac{2}{93555}a^9 \&c. \\ \text{fecant} &= 1 + \frac{1}{2}a^2 + \frac{5}{24}a^4 + \frac{61}{720}a^6 + \frac{277}{8064}a^8 + \frac{50521}{3628800}a^{10} \&c. \\ \text{cofec.} &= a^{-1} + \frac{1}{6}a + \frac{7}{360}a^3 + \frac{31}{15120}a^5 + \frac{127}{604800}a^7 + \frac{73}{3421440}a^9 \&c. \end{aligned}$$

Or the same series are thus otherwise expressed :

$$\begin{aligned} \text{fine} &= a - \frac{1}{2 \cdot 3} a^3 + \frac{b}{4 \cdot 5} a^5 - \frac{c}{6 \cdot 7} a^7 + \frac{d}{8 \cdot 9} a^9 - \frac{e}{10 \cdot 11} a^{11} \&c. \\ \text{cofine} &= 1 - \frac{1}{2} a^2 + \frac{b}{3 \cdot 4} a^4 - \frac{c}{5 \cdot 6} a^6 + \frac{d}{7 \cdot 8} a^8 - \frac{e}{9 \cdot 10} a^{10} \&c. \\ \text{tangent} &= a + \frac{1}{1 \cdot 3} a^3 + \frac{8b}{4 \cdot 5} a^5 + \frac{17c}{6 \cdot 7} a^7 + \frac{29 \frac{3}{17}d}{8 \cdot 9} a^9 + \frac{44 \frac{18}{31}e}{10 \cdot 11} a^{11} \&c. \\ \text{cotang.} &= a^{-1} - \frac{1}{3} a - \frac{b}{15} a^3 - \frac{2c}{21} a^5 - \frac{d}{10} a^7 - \frac{10e}{99} a^9 \&c. \\ \text{fecant} &= 1 + \frac{1}{2} a^2 + \frac{5b}{12} a^4 + \frac{61c}{150} a^6 + \frac{1385d}{3416} a^8 + \frac{50521e}{124651} a^{10} \&c. \\ \text{cofec.} &= a^{-1} + \frac{1}{6} a + \frac{7}{60} a^3 + \frac{31c}{294} a^5 + \frac{127d}{1240} a^7 + \frac{2555e}{25146} a^9 \&c. \end{aligned}$$

where $b, c, d, e, \&c.$ denote the preceding co-efficients. And hence, with the help of the table of the first ten powers of the first 100 numbers, in p. 101. of my tables of powers published by order of the Board of Longitude, may be easily found the fines, &c. of all arcs up to 100, by only dividing those powers by their respective co-efficients, as also of all multiples of these arcs by 10, 100, &c. by only varying the decimal points in the several terms, as the figures will be all the same :
and

and thus a number of primary fines, &c. may be found, to check or verify the same when computed by other methods. By this method will be found the fines, &c. of the arcs

1, 10, 100, 1000, 10000, 100000;

2, 20, 200, 2000, 20000;

3, 30, 300, 3000, 30000;

4, 40, 400, 4000, 40000;

&c. till

99, 990, 9900, 99000, 990000.

13. Again, it is evident, that, of the terms in the series for the sine, the first term a alone will give the sine true to the nearest unit in the ninth place in the first 144 fines, or the arc and sine will be the same for nine places as far as the arc 144; but they will agree to the nearest unit in the seventh place as far as the arc 669; after which the second term of the series must be included;

14. When the second term is taken in, these two terms $a - \frac{1}{2} a^3$ will give the sines true to the nearest unit in the ninth place till the arc becomes 3500. Now the numbers in my table of cubes (just published by order of the Board of Longitude) extend to 10000, and therefore all the above cubes are found in it; consequently taking the sixth part of those cubes, and subtracting it from the corresponding arcs, the remainders will be the sines of those arcs, as far as till the arc be 3500: after which the third term of the series may be taken in, or other methods may be used.

15. But since, for any arc a , this is a general theorem, viz. as radius : $2 \cos. a :: \sin. na : \sin. \overline{n-1} \times a + \sin. \overline{n+1} \times a$; taking $a = 1$, radius 10000, the sine of a will be $1 - .000000000\frac{1}{2}$ and the cosine of a will be $100000 - .000005$, and the
above

above proportion will become $100000 : 200000 - \cdot 00001$, or
 $1 : 2 - \cdot 0000000001 :: \sin. n : \sin. \overline{n-1} + \sin. \overline{n+1}$; conse-
 quently $\sin. \overline{n-1} + \sin. \overline{n+1}$ is $= 2 \sin. n - 0000000001 \sin. n$, and the sines are in arithmetical progression except
 only for the small difference of $\cdot 0000000001 \sin. n$, hence
 $\sin. \overline{n+1}$ is $= 2 - \cdot 0000000001 \times \sin. n - \sin. \overline{n-1}$; and there-
 fore taking n successively equal to 1, 2, 3, 4, &c. the series
 of sines will be as follows:

$$\sin. 1 = 1 - \cdot 0000000000 \frac{1}{8};$$

$$\sin. 2 = 2 - \cdot 0000000001 \times \sin. 1;$$

$$\sin. 3 = 2 - \cdot 0000000001 \times \sin. 2 - \sin. 1;$$

$$\sin. 4 = 2 - \cdot 0000000001 \times \sin. 3 - \sin. 2;$$

$$\sin. 5 = 2 - \cdot 0000000001 \times \sin. 4 - \sin. 3;$$

&c.

And by this theorem, viz. $\sin. \overline{n+1} = 2 - \cdot 0000000001 \times \sin. n - \sin. \overline{n-1}$, may be easily filled up the intervals between those primary numbers mentioned in former articles.

16. In like manner, as $\text{radius} : 2 \cos. a :: \cos. na : \cos. \overline{n-1} . a + \cos. \overline{n+1} . a$; and hence this theorem, $\cos. \overline{n+1} = 2 - \cdot 0000000001 \times \cos. n - \cos. \overline{n-1}$, by which the cosines will be all easily filled up. And these two theorems for the sines and cosines are so easy and accurate, that we need not have recourse to any other, but only to check and verify these at certain intervals, as at every 100th number, by a proportion from RHETICUS's canon, as mentioned at art. 11. or by any other way.

17. The sines and cosines being compleated, the difference between the radius and cosine will be the versed sine; the difference between radius and sine will be the co-versed sine; and the sum of the radius and cosine will be the sup.versed sine.

18. From

18. From the sines and cosines also, the tangents, cotangents, secants, and cosecants, may be made by these known proportions, *viz.* as

1. cosine : radius :: sine : tangent,
2. sine : radius :: cosine : cotangent,
3. cosine : radius :: radius : secant,
4. sine : radius :: radius : cosecant,
5. radius : sine :: secant : tangent,
6. radius : cosine :: cosecant : cotangent,
7. tangent : radius :: radius : cotangent.

Wherefore, the reciprocal of the cosine will be the secant; the reciprocal of the sine, the cosecant; the quotient of the sine by the cosine, the tangent; and the quotient of the cosine by the sine, the cotangent; or the product of the sine and secant will be the tangent, and the product of the cosine and cosecant, the cotangent; or, lastly, the reciprocal of the tangent is the cotangent; proper regard being had to the number of decimals, on account of our radius being 100000 instead of 1 only.

And these are to be used when the application happens to be easier than the general series, and easier than by proportion from RHETICUS's canon.

But there are other particular theorems, which, by a little address, may be rendered more expeditious than any of the former: thus,

19. In any two arcs this is a general proportion,

As the difference of their sines :

to the sum of their sines ::

so tangent of half the difference of the arcs :

to tangent of half their sum.

So that by taking continually the arcs, having the common difference 2, the third term of this proportion will be 1, and the fourth term will be found by dividing the sum of the sines

by their difference, which divisor or difference will never consist of more than four or five figures, *viz.* about half the number of figures that are in the divisors mentioned in the preceding article.

20. Again, As the difference of the cosines :
 to the sum of the cosines ::
 so tangent of half their difference :
 to tangent of half their sum.

And thus the cotangents will be found by dividing the sum of the cosines of two arcs, differing by 2, by their small difference.

21. Also the secant of an arc is equal to the sum of its tangent and the tangent of half its complement; and the cosecant of an arc is equal to the sum of its cotangent and the tangent of half the arc; or half the sum of the tangent and cotangent is equal to the cosecant of the double arc. From whence the secants and cosecants will be easily made.

22. Thus I have pointed out methods by which the whole tables may be readily constructed. Should any other useful methods or improvements occur to any person, the communication of them to me will be thankfully received. I am now engaged in making some of the computations; and it is hoped, that the facility of them, with the desirableness of the tables, will induce some ingenious lovers of the mathematics to lend their aid in performing some part of the work. Should any such be so inclined, before he begins, I must request he will be pleased to signify his intention to me, that I may point out to him such parts of the work as have not before been performed or undertaken, to prevent the chance of losing his labour by re-computing any parts that may have been already executed by myself or others.

Royal Military Academy,
 Woolwich, Aug. 12, 1782.

CHARLES HUTTON.



VII. *On the Means of discovering the Distance, Magnitude, &c. of the Fixed Stars, in consequence of the Diminution of the Velocity of their Light, in case such a Diminution should be found to take place in any of them, and such other Data should be procured from Observations, as would be farther necessary for that Purpose. By the Rev. John Michell, B. D. F. R. S. In a Letter to Henry Cavendish, Esq. F. R. S. and A. S.*

Read November 27, 1783.

DEAR SIR,

Thornhill, May 26, 1783.

THE method, which I mentioned to you when I was last in London, by which it might perhaps be possible to find the distance, magnitude, and weight of some of the fixed stars, by means of the diminution of the velocity of their light, occurred to me soon after I wrote what is mentioned by Dr. PRIESTLEY in his History of Optics, concerning the diminution of the velocity of light in consequence of the attraction of the sun; but the extreme difficulty, and perhaps impossibility, of procuring the other data necessary for this purpose appeared to me to be such objections against the scheme, when I first thought of it, that I gave it then no farther consideration. As some late observations, however, begin to give us a little more chance of procuring some at least of these data, I thought it would not be amiss, that astronomers should be apprized of the method, I propose (which, as far as I know,

has not been suggested by any one else) left, for want of being aware of the use, which may be made of them, they should neglect to make the proper observations, when in their power; I shall therefore beg the favour of you to present the following paper on this subject to the Royal Society.

I am, &c.

THE very great number of stars that have been discovered to be double, triple, &c. particularly by Mr. HERSCHEL *, if we apply the doctrine of chances, as I have heretofore done in my "Enquiry into the probable Parallax, &c. of the Fixed Stars," published in the Philosophical Transactions for the year 1767, cannot leave a doubt with any one, who is properly aware of the force of those arguments, that by far the greatest part, if not all of them, are systems of stars so near to each other, as probably to be liable to be affected sensibly by their mutual gravitation; and it is therefore not unlikely, that the periods of the revolutions of some of these about their principals (the smaller ones being, upon this hypothesis, to be considered as satellites to the others) may some time or other be discovered.

2. Now the apparent diameter of any central body, round which any other body revolves, together with their apparent distance from each other, and the periodical time of the revolv-

* See his Catalogue of Stars of this kind, published in the Philosophical Transactions for the year 1782, which is indeed a most valuable present to the astronomical world. By a happy application of very high magnifying powers to his telescopes, and by a most persevering industry in observing, he has made a very wonderful progress in this branch of astronomy, in which almost nothing of any consequence had been done by any one before him.

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ing body being given, the density of the central body will be given likewise. See Sir ISAAC NEWTON's *Prin.* b. III. pr. VIII. cor. 1.

3. But the density of any central body being given, and the velocity any other body would acquire by falling towards it from an infinite height, or, which is the same thing, the velocity of a comet revolving in a parabolic orbit, at its surface, being given, the quantity of matter, and consequently the real magnitude of the central body, would be given likewise.

4. Let us now suppose the particles of light to be attracted in the same manner as all other bodies with which we are acquainted; that is, by forces bearing the same proportion to their *vis inertiae*, of which there can be no reasonable doubt, gravitation being, as far as we know, or have any reason to believe, an universal law of nature. Upon this supposition then, if any one of the fixed stars, whose density was known by the above-mentioned means, should be large enough sensibly to affect the velocity of the light issuing from it, we should have the means of knowing its real magnitude, &c.

5. It has been demonstrated by Sir ISAAC NEWTON, in the 39th proposition of the first book of his *Principia*, that if a right line be drawn, in the direction of which a body is urged by any forces whatsoever, and there be erected at right angles to that line perpendiculars every where proportional to the forces at the points, at which they are erected respectively, the velocity acquired by a body beginning to move from rest, in consequence of being so urged, will always be proportional to the square root of the area described by the aforesaid perpendiculars. And hence,

6. If such a body, instead of beginning to move from rest, had already some velocity in the direction of the same line, when

when it began to be urged by the aforesaid forces, its velocity would then be always proportional to the square root of the sum or difference of the aforesaid area, and another area, whose square root would be proportional to the velocity which the body had before it began to be so urged; that is, to the square root of the sum of those areas, if the motion acquired was in the same direction as the former motion, and the square root of the difference, if it was in a contrary direction. See cor. 2. to the abovesaid proposition.

7. In order to find, by the foregoing proposition, the velocity which a body would acquire by falling towards any other central body, according to the common law of gravity, let C in the figure (tab. III.) represent the centre of the central body, towards which the falling body is urged, and let CA be a line drawn from the point C , extending infinitely towards A . If then the line RD be supposed to represent the force, by which the falling body would be urged at any point D , the velocity which it would have acquired by falling from an infinite height to the place D would be the same as that which it would acquire by falling from D to C with the force RD , the area of the infinitely extended hyperbolic space $ADRB$, where RD is always inversely proportional to the square of DC , being equal to the rectangle RC contained between the lines RD and CD . From hence we may draw the following corollaries.

8. Cor. 1. The central body DEF remaining the same, and consequently the forces at the same distances remaining the same likewise, the areas of the rectangles RC , rC will always be inversely as the distances of the points D , d from C , their sides RD , rd being inversely in the duplicate ratio of the sides CD , Cd : and therefore, because the velocity of a body falling from an infinite height towards the point C , is always in the sub-

sub-duplicate ratio of these rectangles, it will be in the sub-duplicate ratio of the lines CD , Cd inversely. Accordingly the velocities of comets revolving in parabolic orbits are always in the sub-duplicate ratio of their distances from the sun inversely; and the velocities of the planets, at their mean distances (being always in a given ratio to the velocity of such comets, viz. in the sub-duplicate ratio of 1, to 2) must necessarily observe the same law likewise.

9. Cor. 2. The magnitude of the central body remaining the same, the velocity of a body falling towards it from an infinite height will always be, at the same distance from the point C , taken any where without the central body, in the sub-duplicate ratio of its density; for in this case the distance Cd will remain the same, the line rd only being increased or diminished in the proportion of the density, and the rectangle RC consequently increased or diminished in the same proportion.

10. Cor. 3. The density of the central body remaining the same, the velocity of a body falling towards it from an infinite height will always be as its semi-diameter, when it arrives at the same proportional distance from the point C ; for the weights, at the surfaces of different spheres of the same density are as their respective semi-diameters; and therefore the sides RD and CD , or any other sides rd and Cd , which are in a given ratio to these semi-diameters, being both increased or diminished in the same proportion, the rectangles RC or rc will be increased or diminished in the duplicate ratio of the semi-diameter CD , and consequently the velocity in the simple ratio of CD .

11. Cor. 4. If the velocity of a body falling from an infinite height towards different central bodies is the same, when it arrives at their surfaces, the density of those central bodies must be

in the duplicate ratio of their semi-diameters inversely; for by the last cor. the density of the central body remaining the same, the rectangle RC will be in the duplicate ratio of CD; in order therefore that the rectangle RC may always remain the same, the line RD must be inversely, as CD, and consequently the density inversely, as the square of CD.

12. Cor. 5. Hence the quantity of matter contained in those bodies must be in the simple ratio of their semi-diameters directly; for the quantity of matter being always in a ratio compounded of the simple ratio of the density, and the triplicate ratio of their semi-diameters, if the density is in the inverse duplicate ratio of the semi-diameters, this will become the direct triplicate and inverse duplicate, that is, when the two are compounded together, the simple ratio of the semi-diameters,

13. The velocity a body would acquire by falling from an infinite height towards the sun, when it arrived at his surface, being, as has been said before in article 3d, the same with that of a comet revolving in a parabolic orbit in the same place, would be about 20,72 times greater than that of the earth in its orbit at its mean distance from the sun; for the mean distance of the earth from the sun, being about 214,64 of the sun's semidiameters, the velocity of such a comet would be greater at that distance than at the distance of the earth from the sun, in the sub-duplicate ratio of 214,64 to 1, and the velocity of the comet being likewise greater than that of planets, at their mean distances, in the sub-duplicate ratio of 2 to 1; these, when taken together, will make the sub-duplicate ratio of 429,28 to 1, and the square root of 429,28 is 20,72, very nearly.

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14. The same result would have been obtained by taking the line RD proportional to the force of gravity at the sun's surface, and DC equal to his semi-diameter, and from thence computing a velocity, which should be proportional to the square root of the area RC when compared with the square root of another area, one of whose sides should be proportional to the force of gravity at the surface of the earth; and the other should be, for instance, equal to 16 feet, 1 inch, the space a body would fall through in one second of time, in which case it would acquire a velocity of 32 feet, 2 inches per second. The velocity thus found compared with the velocity of the earth in its orbit, when computed from the same elements, necessarily gives the same result. I have made use of this latter method of computation upon a former occasion, as may be seen in Dr. PRIESTLEY's History of Optics, p. 787, &c. but I have rather chosen to take the velocity from that of a comet, in the article above, on account of its greater simplicity, and its more immediate connexion with the subject of this paper.

15. The velocity of light, exceeding that of the earth in its orbit, when at its mean distance from the sun, in the proportion of about 10.310 to 1, if we divide 10.310 by 20,72, the quotient 497, in round numbers, will express the number of times, which the velocity of light exceeds the velocity a body could acquire by falling from an infinite height towards the sun, when it arrived at his surface; and an area whose square root should exceed the square root of the area RC, where RD is supposed to represent the force of gravity at the surface of the sun, and CD is equal to his semi-diameter, in the same proportion, must consequently exceed the area RC in the proportion of 247.009, the square of 497 to 1.

16. Hence, according to article 10, if the semi-diameter of a sphere of the same density with the sun were to exceed that of the sun in the proportion of 500 to 1, a body falling from an infinite height towards it, would have acquired at its surface a greater velocity than that of light, and consequently, supposing light to be attracted by the same force in proportion to its vis inertię, with other bodies, all light emitted from such a body would be made to return towards it, by its own proper gravity.

17. But if the semi-diameter of a sphere, of the same density with the sun, was of any other size less than 497 times that of the sun, though the velocity of the light emitted from such a body, would never be wholly destroyed, yet would it always suffer some diminution, more or less, according to the magnitude of the said sphere; and the quantity of this diminution may be easily found in the following manner: Suppose S to represent the semi-diameter of the sun, and aS to represent the semi-diameter of the proposed sphere; then, as appears from what has been shewn before, the square root of the difference between the square of 497 S and the square of aS will be always proportional to the ultimately remaining velocity, after it has suffered all the diminution, it can possibly suffer from this cause; and consequently the difference between the whole velocity of light, and the remaining velocity, as found above, will be the diminution of its velocity. And hence the diminution of the velocity of light emitted from the sun, on account of it's gravitation towards that body, will be somewhat less than a 494.000th part of the velocity which it would have had if no such diminution had taken place; for the square of 497 being 247.009, and the square of 1 being 1, the diminution of the velocity will be the difference between

the square root of 247.009, and the square root of 247.008, which amounts, as above, to somewhat less than one 494.000th part of the whole quantity.

18. The same effects would likewise take place, according to article 11, if the semi-diameters were different from those mentioned in the two last articles, provided the density was greater or less in the duplicate ratio of those semi-diameters inversely.

19. The better to illustrate this matter, it may not be amiss to take a particular example. Let us suppose then, that it should appear from observations made upon some one of those double stars above alluded to, that one of the two performed its revolution round the other in 64 years, and that the central one was of the same density with the sun, which it must be, if its apparent diameter, when seen from the other body, was the same as the apparent diameter of the sun would be if seen from a planet revolving round him in the same period: let us further suppose, that the velocity of the light of the central body was found to be less than that of the sun, or other stars whose magnitude was not sufficient to affect it sensibly, in the proportion of 19 to 20. In this case then, according to article 17, the square root of 247.009 SS must be to the square root of the difference between 247.009 SS and aaSS as 20 to 19. But the squares of 20 and 19 being 400 and 361, the quantity 247.009 SS must therefore be to the difference between this quantity and aaSS in the same proportion, that is as 247.009 to 222.925,62; and aaSS must consequently be equal to 24.083, 38 SS, whose square root 155,2 S nearly, or, in round numbers, 155 times the diameter of the sun, will be the diameter of the central star sought.

20. As the squares of the periodical times of bodies, revolving round a central body, are always proportional to the cubes of their mean distances, the distance of the two bodies from each other must therefore, upon the foregoing suppositions, be sixteen times greater in proportion to the diameter of the central body, than the distance of the earth from the sun in proportion to his diameter; and that diameter being already found to be also greater than that of the sun in the proportion of 155,2 to 1, this distance will consequently be greater than that of the earth and sun from each other in the proportion of 16 times 155,2, that is 2483,2 to 1.

21. Let us farther suppose, that from the observations, the greatest distance of the two stars in question appeared to be only one second; we must then multiply the number 2483,2 by 206.264,8, the number of seconds in the radius of a circle, and the product 512.196.750 will shew the number of times which such a star's distance from us must exceed that of the sun. The quantity of matter contained in such a star would be $\frac{1}{155,2}$ or 3.738.308 times as much as that contained in the sun; its light, supposing the sun's light to take up 8'. 7". in coming to the earth, would, with its common velocity, require 7.900 years to arrive at us, and 395 years more on account of the diminution of that velocity; and supposing such a star to be equally luminous with the sun, it would still be very sufficiently visible, I apprehend, to the naked eye, notwithstanding its immense distance.

22. In the elements which I have employed in the above computations, I have supposed the diameter of the central star to have been observed, in order to ascertain its density, which cannot be known without it; but the diameter of such a star is

much

much too small to be observed by any telescopes yet existing, or any that it is probably in the power of human abilities to make; for the apparent diameter of the central star, if of the same density with the sun, when seen from another body, which would revolve round it in 64 years, would be only the 1717th part of the distance of those bodies from each other, as will appear from multiplying 107,32, the number of times the sun's diameter is contained in his distance from the earth, by 16, the greater proportional distance of the revolving body, corresponding to 64 years instead of 1. Now the 1717th part of a second must be magnified 309.060 times in order to give it an apparent diameter of three minutes; and three minutes, if the telescopes were mathematically perfect, and there was no want of distinctness in the air, would be but a very small matter to judge of*.

23. But

* In Mr. HERSCHEL's Observations upon the Fixed Stars abovementioned, almost all of them are represented as appearing with a well-defined round disc. That this is not the real disc, but only an optical appearance, occasioned perhaps by the constitution of the eye, when the pencil, by which objects are seen, is so exceedingly small as those which he employed upon this occasion, is very manifest, from the observations themselves, of which indeed Mr. HERSCHEL seems to be himself sufficiently aware: if it were not so, the intensity of the light of these stars must either be exceedingly inferior indeed to that of the sun, or they must be immensely larger, otherwise they must have a very sensible parallax; for the sun, if removed 10 10.000.000 times his present distance, would still, I apprehend, be of about the brightness of the stars of the sixth magnitude; in which case he must be magnified 1.000.000 times to make his apparent disc of any sensible magnitude; or, on the other hand, if he was only removed to a thousandth part of that distance, then he must be less luminous in the proportion of 1.000.000 to 1, to make him appear no brighter than a star of the sixth magnitude. Now the sun's diameter being contained nearly 215 times in the diameter of the earth's orbit, the annual parallax therefore of such a body in that case, if it was placed in the pole of the ecliptic,

23. But though there is not the least probability that this element, so essential to be known, in order to determine with precision the exact distance and magnitude of a star, can ever be obtained, where it is in the same circumstances, or nearly the same, with those above supposed, yet the other elements, such as perhaps may be obtained, are sufficient to determine the distance, &c. with a good deal of probability, within some moderate limits; for in whatever ratio the real distance of the two stars may be greater or less than the distance supposed, the density of the central star must be greater or less in the sixth power of that ratio inversely; for the periodic time of the revolving body being given, the quantity of matter contained in the central body must be as the cube of their distance from each other. See Sir I. NEWTON's Prin. b. 3d. pr. 8th. cor 3d. But the quantity of matter in different bodies, at whose surfaces the velocity acquired by falling from an infinite height is the same, must be, according to art. 12, directly as their semi-diameters; the semi-diameters therefore of such bodies must be in the triplicate ratio of the distance of the revolving body; and consequently their densities, by art. 11, being in the inverse duplicate ratio of their semi-diameters, must be in the inverse sextuplicate ratio of the distance of the revolving body. Hence if the real distance should be greater or less than that supposed, in the proportion of two or three to one, the density of the central body must be less or greater, in the first case, in the proportion of 64, or in the latter of 729 to 1.

ecliptic, would be 215 times its apparent diameter; and as the bright star in Lyræ appeared to Mr. HERSCHEL about a third part of a second in diameter, if this was its real disc, and it was no bigger than the sun, it would consequently have an annual parallax in the pole of the ecliptic of about 72".

24. There

24. There is also another circumstance, from which perhaps some little additional probability might be derived, with regard to the real distance of a star, such as that we have supposed; but upon which however, it must be acknowledged, that no great stress can be laid, unless we had some better analogy to go upon than we have at present. The circumstance I mean is the greater specific brightness which such a star must have, in proportion as the real distance is less than that supposed, and *vice versa*; since, in order that the star may appear equally luminous, its specific brightness must be as the fourth power of its distance inversely; for the diameter of the central star being as the cube of the distance between that and the revolving star, and their distance from the earth being in the simple ratio of their distance from each other, the apparent diameter of the central star must be as the square of its real distance from the earth, and consequently, the surface of a sphere being as the square of its diameter, the area of the apparent disc of such a star must be as the fourth power of its distance from the earth; but in whatever ratio the apparent disc of the star is greater or less, in the same ratio inversely must be the intensity of its light, in order to make it appear equally luminous. Hence, if its real distance should be greater or less than that supposed in the proportion of 2 or 3 to 1, the intensity of its light must be less or greater, in the first case, in the proportion of 16, or, in the latter of 81 to 1.

25. According to Mons. BOUGUER (see his *Traité d'Optique*) the brightness of the sun exceeds that of a wax candle in no less a proportion than that of 8000 to 1. If therefore the brightness of any of the fixed stars should not exceed that of our common candles, which, as being something less luminous than
wax,

wax, we will suppose in round numbers to be only one 10.000th part as bright as the sun, such a star would not be visible at more than an 1000th part of the distance, at which it would be visible, if it was as bright as the sun. Now because the sun would still appear, I apprehend, as luminous, as the star Sirius, when removed to 400.000 times his present distance, such a body, if no brighter than our common candles, would only appear equally luminous with that star at 4000 times the distance of the sun, and we might then begin to be able, with the best telescopes, to distinguish some sensible apparent diameter of it; but the apparent diameters of the stars of the less magnitudes would still be too small to be distinguishable even with our best telescopes, unless they were yet a good deal less luminous, which may possibly however be the case with some of them; for, though we have indeed very slight grounds to go upon with regard to the specific brightness of the fixed stars compared with that of the sun at present, and can therefore only form very uncertain and random conjectures concerning it, yet from the infinite variety which we find in the works of the creation, it is not unreasonable to suspect, that very possibly some of the fixed stars may have so little natural brightness in proportion to their magnitude, as to admit of their diameters having some sensible apparent size, when they shall come to be more carefully examined, and with larger and better telescopes than have been hitherto in common use.

26. With regard to the sun, we know that his whole surface is extremely luminous, a very small and temporary interruption sometimes from a few spots only excepted. This universal and excessive brightness of the whole surface is probably owing to an atmosphere, which being luminous throughout,
and

and in some measure also transparent, the light, proceeding from a considerable depth of it, all arrives at the eye; in the same manner as the light of a great number of candles would do, if they were placed one behind another, and their flames were sufficiently transparent to permit the light of the more distant ones to pass through those that were nearer, without any interruption.

27. How far the same constitution may take place in the fixed stars we don't know; probably however it may do so in many; but there are some appearances with regard to a few of them, which seem to make it probable, that it does not do so universally. Now, if I am right in supposing the light of the sun to proceed from a luminous atmosphere, which must necessarily diffuse itself equally over the whole surface, and I think there can be very little doubt that this is really the case, this constitution cannot well take place in those stars, which are in some degree periodically more and less luminous, such as that in *Collo Ceti*, &c. It is also not very improbable, that there is some difference from that of the sun, in the constitution of those stars, which have sometimes appeared and sometimes disappeared, of which that in the constellation of *Cassiopeia* is a notable instance. And if those conjectures are well founded which have been formed by some philosophers concerning stars of these kinds, that they are not wholly luminous, or at least not constantly so, but that all, or by far the greatest part of their surfaces is subject to considerable changes, sometimes becoming luminous, and at other times being extinguished; it is amongst the stars of this sort, that we are most likely to meet with instances of a sensible apparent diameter, their light being much more likely not to be so great in proportion as that of the sun, which, if removed to four hundred thousand times

his present distance would still appear, I apprehend, as bright as Sirius, as I have observed above; whereas it is hardly to be expected, with any telescopes whatsoever, that we should ever be able to distinguish a well defined disc of any body of the same size with the sun at much more than ten thousand times his distance.

28. Hence the greatest distance at which it would be possible to distinguish any sensible apparent diameter of a body as dense as the sun cannot well greatly exceed five hundred times ten thousand, that is, five million times the distance of the sun; for if the diameter of such a body was not less than five hundred times that of the sun, its light, as has been shewn above, in art. 16. could never arrive at us.

29. If there should really exist in nature any bodies, whose density is not less than that of the sun, and whose diameters are more than 500 times the diameter of the sun, since their light could not arrive at us; or if there should exist any other bodies of a somewhat smaller size; which are not naturally luminous; of the existence of bodies under either of these circumstances, we could have no information from sight; yet, if any other luminous bodies should happen to revolve about them, we might still perhaps from the motions of these revolving bodies infer the existence of the central ones with some degree of probability, as this might afford a clue to some of the apparent irregularities of the revolving bodies, which would not be easily explicable on any other hypothesis; but as the consequences of such a supposition are very obvious, and the consideration of them somewhat beside my present purpose, I shall not prosecute them any farther.

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30. The diminution of the velocity of light, in case it should be found to take place in any of the fixed stars, is the principal phenomenon whence it is proposed to discover their distance, &c. Now the means by which we may find what this diminution amounts to, seems to be supplied by the difference which would be occasioned in consequence of it, in the refrangibility of the light, whose velocity should be so diminished. For let us suppose with Sir ISAAC NEWTON (see his Optics, prop. vi. paragr. 4 and 5) that the refraction of light is occasioned by a certain force impelling it towards the refracting medium, an hypothesis which perfectly accounts for all the appearances. Upon this hypothesis the velocity of light in any medium, in whatever direction it falls upon it, will always bear a given ratio to the velocity it had before it fell upon it, and the sines of incidence and refraction will, in consequence of this, bear the same ratio to each other with these velocities inversely. Thus, according to this hypothesis, if the sines of the angles of incidence and refraction, when light passes out of air into glass, are in the ratio of 31 to 20, the velocity of light in the glass must be to its velocity in air in the same proportion of 31 to 20. But because the areas, representing the forces generating these velocities, are as the squares of the velocities, see art. 5. and 6. these areas must be to each other as 961 to 400. And if 400 represents the area which corresponds to the force producing the original velocity of light, 561, the difference between 961 and 400, must represent the area corresponding to the additional force, by which the light was accelerated at the surface of the glass.

31. In art. 19. we supposed, by way of example, the velocity of the light of some particular star to be diminished in the

ratio of 19 to 20, and it was there observed, that the area representing the remaining force which would be necessary to generate the velocity 19, was therefore properly represented by $\frac{3}{4} \frac{6}{10}$ th parts of the area, that should represent the force that would be necessary to generate the whole velocity of light, when undiminished. If then we add 561, the area representing the force by which the light is accelerated at the surface of the glass, to 361, the area representing the force which would have generated the diminished velocity of the star's light, the square root of 922, their sum, will represent the velocity of the light with the diminished velocity, after it has entered the glass. And the square root of 922 being 30,364, the sines of incidence and refraction of such light out of air into glass will consequently be as 30,364 to 19, or what is equal to it, as 31,96 to 20 instead of 31 to 20, the ratio of the sines of incidence and refraction, when the light enters the glass with its velocity undiminished.

32. From hence a prism, with a small refracting angle, might perhaps be found to be no very inconvenient instrument for this purpose: for by such a prism, whose refracting angle was of one minute, for instance, the light with its velocity undiminished would be turned out of its way 33'', and with the diminished velocity 35'', 88 nearly, the difference between which being almost 2''. 53'', would be the quantity by which the light, whose velocity was diminished, would be turned out of its way more than that whose velocity was undiminished.

33. Let us now be supposed to make use of such a prism to look at two stars, under the same circumstances as the two stars in the example above-mentioned, the central one of which should be large enough to diminish the velocity of its light one twentieth part, whilst the velocity of the light of the other, which

which was supposed to revolve about it as a satellite, for want of sufficient magnitude in the body from whence it was emitted, should suffer no sensible diminution at all. Placing then the line, in which the two faces of the prism would intersect each other, at right angles to a line joining the two stars; if the thinner part of the prism lay towards the same point of the heavens with the central star, whose light would be most turned out of its way, the apparent distance of the stars would be increased $2''.53'''$ and consequently become $3''.53'''$ instead of $1''$. only, the apparent distance supposed above in art. 21. On the contrary, if the prism should be turned half way round, and its thinner part lay towards the same point of the heavens with the revolving star, their distance must be diminished by a like quantity, and the central star therefore would appear $1''.53'''$ distant from the other on the opposite side of it, having been removed from its place near three times the whole distance between them.

34. As a prism might be made use of for this purpose, which should have a much larger refracting angle than that we have proposed, especially if it was constructed in the achromatic way, according to Mr. DOLLOND's principles, not only such a diminution, as one part in twenty, might be made still more distinguishable; but we might probably be able to discover considerably less diminutions in the velocity of light, as perhaps a hundredth, a two-hundredth, a five-hundredth, or even a thousandth part of the whole, which, according to what has been said above, would be occasioned by sphæres, whose diameters should be to that of the sun, provided they were of the same density, in the several proportions nearly of 70, 50, 30, and 22 to 1 respectively.

35. If such a diminution of the velocity of light, as that above supposed, should be found really to take place, in consequence.

quence of its gravitation towards the bodies from whence it is emitted, and there should be several of the fixed stars large enough to make it sufficiently sensible, a set of observations upon this subject might probably give us some considerable information with regard to many circumstances of that part of the universe, which is visible to us. The quantity of matter contained in many of the fixed stars might from hence be judged of, with a great degree of probability, within some moderate limits; for though the exact quantity must still depend upon their density, yet we must suppose the density most enormously different from that of the sun, and more so, indeed, than one can easily conceive to take place in fact, to make the error of the supposed quantity of matter very wide of the truth, since the density, as has been shewn above in art. 11. and 12. which is necessary to produce the same diminution in the velocity of light, emitted from different bodies, is as the square of the quantity of matter contained in those bodies inversely.

36. But though we might possibly from hence form some reasonable guess at the quantity of matter contained in several of the fixed stars; yet, if they have no luminous satellites revolving about them, we shall still be at a loss to form any probable judgment of their distance, unless we had some analogy to go upon for their specific brightness, or had some other means of discovering it; there is, however, a case that may possibly occur, which may tend to throw some light upon this matter.

37. I have shewn in my Enquiry into the probable Parallax, &c. of the Fixed Stars, published in the Philosophical Transactions for the year 1767, the extremely great probability there is, that many of the fixed stars are collected together into groups; and that the Pleiades in particular constitute one of these

these groups. Now of the stars which we there see collected together, it is highly probable, as I have observed in that paper, that there is not one in a hundred which does not belong to the group itself; and by far the greatest part, therefore, according to the same idea, must lye within a sphere, a great circle of which is of the same size with a circle, which appears to us to include the whole group. If we suppose, therefore, this circle to be about 2° . in diameter, and consequently only about a thirtieth part of the distance at which it is seen, we may conclude, with the highest degree of probability, that by far the greatest part of these stars do not differ in their distances from the sun by more than about one part in thirty, and from thence deduce a sort of scale of the proportion of the light which is produced by different stars of the same group or system in the Pleiades at least; and, by a somewhat probable analogy, we may do the same in other systems likewise. But having yet no means of knowing their real distance, or specific brightness, when compared either with the sun or with one another, we shall still want something more to form a farther judgment from.

38. If, however, it should be found, that amongst the Pleiades, or any other like system, there are some stars that are double, triple, &c. of which one is a larger central body, with one or more satellites revolving about it, and the central body should likewise be found to diminish the velocity of its light; and more especially, if there should be several such instances met with in the same system; we should then begin to have a kind of measure both of the distance of such a system of stars from the earth, and of their mutual distances from each other. And if several instances of this kind should occur in different groups or systems of stars, we might also, perhaps, begin to form

form some probable conjectures concerning the specific density and brightness of the stars themselves, especially if there should be found any general analogy between the quantity of the diminution of the light and the distance of the system deduced from it; as, for instance, if those stars, which had the greatest effect in diminishing the velocity of light should in general give a greater distance to the system, when supposed to be of the same density with the sun, we might then naturally conclude from thence, that they are less in bulk, and of greater specific density, than those stars which diminish the velocity of light less, and *vice versa*. In like manner, if the larger stars were to give us in general a greater or less quantity of light in proportion to their bulk, this would give us a kind of analogy, from whence we might perhaps form some judgment of the specific brightness of the stars in general; but, at all adventures, we should have a pretty tolerable measure of the comparative brightness of the sun and those stars, upon which such observations should be made, if the result of them should turn out agreeable to the ideas above explained.

39. Though it is not improbable, that a few years may inform us, that some of the great number of double, triple stars, &c. which have been observed by Mr. HERSCHEL, are systems of bodies revolving about each other, especially if a few more observers, equally ingenious and industrious with himself could be found to second his labours; yet the very great distance at which it is not unlikely many of the secondary stars may be placed from their principals, and the consequently very long periods of their revolutions*, leave very little room to hope that

* If the sun, when removed to 10,000,000 times his present distance, would still appear as bright as a star of the sixth magnitude, which I apprehend to be pretty

that any very great progress can be made in this subject for many years, or perhaps some ages to come; the above outlines, therefore, of the use that may be made of the observations upon the double stars, &c. provided the particles of light should be subject to the same law of gravitation with other bodies, as in all probability they are, and provided also that some of the stars should be large enough sensibly to diminish their velocity, will, I hope, be an inducement to those, who may have it in their power, to make these observations for the benefit of future generations at least, how little advantage soever we may expect from them ourselves; and yet very possibly some observations of this sort, and such as may be made in a few years, may not only be sufficient to do something, even at present, but also to shew, that much more may be done hereafter, when these observations shall become more numerous, and have been continued for a longer period of years.

pretty near the truth, any satellite revolving round such a star, provided the star was not either of less specific brightness, or of greater density than the sun, must, if it appeared at its greatest elongation, at the distance of one second only from its principal, be between three and four hundred years in performing one revolution; and the time of the revolution of the very small star near α Lyrae, if it is a satellite to this latter, and its principal is of the same specific brightness and density with the sun, could hardly be less than eight hundred years, though 37'' the distance at which it is placed from it, according to Mr. HERSCHEL's observations, should happen to be its greatest distance. These periodical times, however, are computed from the above distances, upon the supposition of the star, that revolves as a satellite, being very much smaller than the central one, so as not to disturb its place sensibly; for if the two stars should contain equal, or nearly equal, quantities of matter, the periodical times might be somewhat less, on account of their revolving about their common centre of gravity, in circles of little more than half as great a diameter as that in which the satellite must revolve upon the other supposition.

VIII. *A Meteorological Journal for the Year 1782, kept at Minehead, in Somersetshire. By Mr. John Atkins; communicated by Sir Joseph Banks, Bart. P. R. S.*

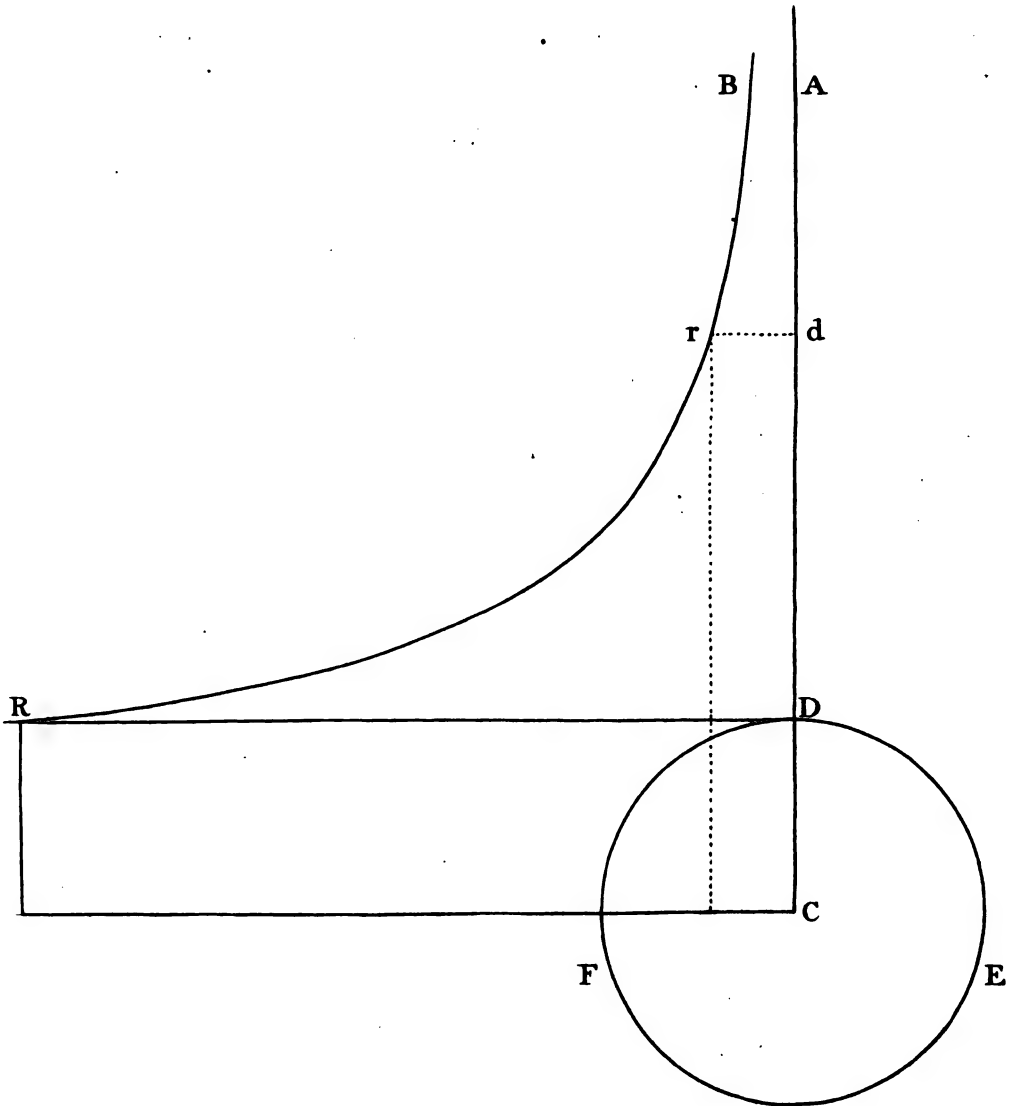
Read January 15, 1784.

S I R,

Minehead, March 27, 1783.

THE numberless philosophical discoveries and meteorological journals which I find have been addressed to you, as being a gentleman whose great abilities have raised you to the highest pitch of grandeur in the philosophical world, and which, I find, have been treated with the greatest candour and respect, and published under your direction for the improvement of scientific knowledge, make me presume (though an unknown, and even unheard of, individual) to direct this journal to you, not boasting, but rather doubting, of its being worthy of your reception; but my having found so great a difference between the last year and several preceding years, in the variations of the atmosphere, both barometrical and thermometrical, induced me to communicate it to other observers through your approbation.

I am, &c.



THE instruments are kept at a house about thirty feet above high water in the Bristol Channel. The barometer is made after DE LUC's method; and to observe the most minute alteration I have divided it into the one-sixteenth of a line, or 192 parts in an inch. The thermometer is a mercurial one, graduated according to FAHRENHEIT's scale, as being the most universal, though, I think, a partial one, and placed in the open air in a northern aspect. An hygrometer I have likewise kept in the open air; but being an instrument that does not admit of, as I ever heard of, a certain basis, whereon to fix the fundamental point between the greatest moisture and greatest drought, and therefore of little use to distant observers, I have omitted these observations: For the ease of correspondent observers, I have drawn two columns of the barometer; the first divided into 192d parts of an inch; the second into 100th parts. The figures in the column of winds denote its strength from 0 to 90 degrees of a quadrant. And the most prevailing winds are from north to west, being generally in those directions two-thirds of the year, occasioned, as I imagine, by the indraught of the Bristol Channel. The barometer this year has taken a greater range than ever I found these several years, being 2.44 inches. The thermometer likewise from 21° to 81° ; and the three rainy months of October, November, and December, there fell very little more rain than fell in the month of August alone, which is very uncommon in this part of the kingdom. On the ninth of February an odd phænomenon appeared to me about 10 miles from hence, on my journey to Tiverton: I observed an halo, exactly similar to that of the sun, the center of the arch about 15° high, and both ends terminated in a field of snow; but as rainbows are seen only with the sun behind one's back, this, on the contrary, was between me and the sun.

January 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.		Inch.
1	8	Hard rain.	SW 25		29.96	29.50	50	0.55
	12				29.100	29.52	52	
	9		W 20		29.104	29.54	50	
2	8	Rain.	W 30		29.16	29.9	48	0.16
	12				29.120	29.63	50	
	9				29.128	29.67	50	
3	8	Fair. Some showers.	WNW 35		29.120	29.63	48	
	12				29.140	29.73	50	
	9				29.152	29.79	48	
4	8	Foggy, rain.	WNW 10		29.160	29.84	50	0.17
	12				29.140	29.73	54	
	9				29.128	29.67	49	
5	8	Showers.	WNW 27		29.132	29.69	50	0.25
	12				29.150	29.78	51	
	9				29.170	29.89	50	
6	8	Fair. Cloudy.	WNW 22		29.184	29.94	46	
	12				30. 0	30. 0	50	
	9				30. 0	30. 0	49	
7	8	Hard showers. Stormy, hail showers.	NW 40		29.112	29.59	45	
	12				29.120	29.63	50	
	9		70		29.128	29.67	42	
8	8	Fair. Showers.	WSW 20		29.158	29.83	44	0.50
	12				29.150	29.78	50	
	9				29.128	29.67	48	
9	8	Rain. Very stormy.	S 40		29. 0	29. 0	50	
	12		WNW 50		28.168	28.88	40	
	9		NW 80		29. 40	29.21	38	

January 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom. °	Rain.
					Inches.	Inches.		Inch.
10	8	Very stormy.	NW by W 80.		29.128	29.67	37	
	12				29.180	29.94	40	0.49
	9	Freezing hard.			30. 48	30.25	35	
11	8	Hard frost and fair.	NW 0.		30. 48	30.25	29	
	12				30. 48	30.25	32	
	9				30. 48	30.25	35	
12	8	Foggy rain.	NW 0.		30. 40	30.21	45	
	11				30. 40	30.21	48	
	9				30. 64	30.33	49	
13	8	Cloudy.	WNW 0.		30. 70	30.37	47	
	12	Foggy rain.			30. 70	30.37	51	
	9				30. 67	30.35	50	
14	8	Fair.	S by W 15.		30. 56	30.29	45	
	12				30. 40	30.21	50	
	9				30. 24	30.13	41	
15	8	Frosty, but foggy.	NNW 35.		30. 20	30.11	40	
	12				30. 28	30.15	44	
	9				30. 16	30. 9	44	
16	8	Small rain.	WNW 10.		29.128	29.67	47	
	12				29.106	29.55	50	
	9	Stormy, hail showers.	NW 75		29. 80	29.42	42	
17	8	Stormy, but dry.	NW 70		29. 88	29.46	39	
	12				29. 96	29.50	43	
	9				29.112	29.59	45	
18	8	Fair.	SW 20		29.118	29.61	48	
	12	Some small rain.			29.126	29.66	48	
	9				29.144	29.75	45	

January

January 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom. °	Rain. Inch.
					Inches.	Inches.		
19	8	Cloudy.	WNW 15		29.140	29.73	45	
	12	Fair and mild.			29.140	29.73	51	
	9	Some showers.			29.130	29.68	46	
20	8	Very hard rain.	W 60		29. 90	29.47	50	0.66
	12				29. 80	29.42	52	
	9				29. 86	29.45	48	
21	8	Frost.	WNW 10		29.140	29.73	38	
	12	Some rain, but snow on the hills.			29.168	29.88	45	
	9				30. 0	30. 0	48	
22	8	Flying clouds, with some showers.	W 60		29.180	29.94	51	
	12				29.180	29.94	51	
	9	Cloudy and mild.			29.180	29.94	50	
23	8	Fair and mild.	W 60		29.184	29.96	50	
	12				29.180	29.94	51	
	9				29.176	29.92	50	
24	8	{ Fair, with hard winds, but very mild.	W 70		29.120	29.63	50	
	12				29.112	29.59	55	
	9	Calm, with rain.			29.116	29.61	49	
25	8	Stormy, hail showers.	NW 75		29.120	29.63	36	
	12				29.130	29.68	39	
	9	Rain.			29.100	29.52	44	
26	8	Fair.	NW 60		29.116	29.61	39	
	12	Hail showers.			29.110	29.57	41	
	9	Stormy, hard rain.			29.100	29.52	40	
27	8	Stormy, hail and rain.	N by W 60		29. 48	29.25	38	0.93
	12				29. 40	29.21	42	
	9				29. 48	29.25	40	

January

January 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Therm.	Rain.
					Inches.	Inches.	°	Inch.
28	8	Stormy, showers.	NW by W 80		29.32	29.17	40	
	12				29.16	29.9	45	
	9	Calmer, with hail in the night.	20		29.16	29.9	40	0.23
29	8	Fair and cold.	W 20		28.176	28.92	33	
	12	Fair.	NW		28.160	28.84	41	
	5		SW		28.154	28.81		
	9	Cloudy.	NE	SW	29.0	29.0	37	
30	8	{ Cloudy, but snow on the distant hills.	NE 20		29.64	29.34	37	
	12	Fair.			29.112	29.59	40	
	9	Freezing hard.			29.168	29.88	35	
31	8	Frost.	NE 15		30.16	30.9	33	
	12				30.16	30.9	37	
	9				30.16	30.9	32	
Total rain								3.94

February

February 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom. °	Rain.
					Inches.	Inches.		Inch.
1	7	Very hard frost.	W 0		30. 0	30. 0	27	
	12	Fair.	SW 20		29.180	29.94	34	
	9	Sleet.			29.160	29.84	37	
2	7	Rain.	SW 30		29.130	29.68	40	
	12				29.120	29.63	42	
	9	Fair.			29.112	29.59	38	
3	7	Frost.	S by E 20	SW	29. 96	29.50	35	
	12	Sleet.	SW		29. 84	29.44	37	
	9				29. 80	29.42	37	
4	7	Fair.	W 0		29. 80	29.42	37	
	12				29. 88	29.46	38	
	9	Snow on the hills.	NE 20		29. 60	29.31	37	
5	7	Cloudy day.	NE 40		29. 12	29. 6	33	0.63
	12				29. 20	29.11	35	
	9				29. 96	29.50	35	
6	7	Fair, frost.	NE 20		29.140	29.73	35	
	12				29.160	29.84	37	
	9		W 0		29.168	29.88	37	
7	7	Fair, hard frost.	W 0	ENE	29.170	29.89	29	
	12				29.172	29.90	35	
	9		NE 10		29.180	29.94	35	
8	7	Fair, frost.	NE 0		29.180	29.94	34	
	12				29.180	29.94	37	
	9				29.186	29.97	33	
9	7	{ Fair, very hard frost; an extraordinary halo.	NE 10		29.182	29.95	27	
	12	Cloudy.			29.180	29.94	33	
	9				29.180	29.94	33	

February

Sept at Minehead, in Somersetshire.

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February 1782.

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K

February

February 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.		Inch.
19	7	Cloudy, a little frost.	E 10		30. 60	30.31	35	
	12				30. 57	30.30	37	
	9				30. 48	30.25	37	
20	7	Cloudy.	W 10		30. 40	30.21	34	
	12				30. 40	30.21	39	
	9		WNW		29.180	29.94	37	
21	7	Fair.	S by W 0		29.160	29.84	37	
	12				29.144	29.75	41	
	9	Rain.			29. 32	29.17	40	
22	7	Fair.	W 30		29. 48	29.25	41	
	12				29. 40	29.21	44	
	9	Cloudy.			29. 32	29.17	40	
23	7	Hard rain and wind.	S 80		29. 0	29. 0	46	
	12				28.168	28.88	49	
	4				28.160	28.84		
	9		WNW 40		28.170	28.89	46	
24	7	Fair.	WNW 30		29. 30	29.16	45	0.67
	12				29.100	29.52	47	
	9	Rain in the night.			29.168	29.88	40	
25	7	Fair.	W by N 35		29.172	29.90	45	
	12				30. 12	30. 6	48	
	9				30. 0	30. 0	47	
26	7	Small showers.	W 75		29.186	29.97	48	
	12	Fair and mild.			30. 0	30. 0	61	0.24
	9				30. 0	30. 0	57	
27	7	Cloudy.	S 5		29.180	29.94	50	
	12				29.180	29.94	52	
	9	Fair.			29.184	29.96	45	

JANUARY

1

FEBRUARY

February 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
28	7	Fair.	W 25		30. 40	30.21	45	
12	30. 48				30.25	48		
9	30. 32				30.17	44		
					Total rain 1.54			

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March

March 1782

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
1	6	Fair.	W 10		30. 0	30. 0	42	
	12				29.186	29.97	45	
	9		40		29.174	29.91	40	
2	6	Fair.	WNW 15		29.170	29.89	38	
	12				29.176	29.92	48	
	9	Cloudy.	NW		29.180	29.94	40	
3	6	Fair, white frost.	NW 0		29.186	29.97	39	
	12				30. 0	30. 0	47	
	9		20		30. 16	30. 9	40	
4	6	Hard frost.	W 0		30. 30	30.16	32	
	12				30. 16	30. 9	37	
	9		20		30. 16	30. 9	37	
5	6	Little frost.	WSW 30		29.178	29.93	38	
	12				29.170	29.89	45	
	9				29.172	29.90	40	
6	6	Fair.	WNW 10		29.168	29.88	40	
	12				29.168	29.88	50	
	9				29.164	29.86	47	
7	6	Fair.	W 20		29.158	29.83	40	
	12				29.150	29.78	44	
	9	Showers.	75		29.144	29.75	48	
8	6	Sleet, but snow on the hills.	SSW 70		29.144	29.75	39	
	12		50		29. 32	29.17	40	
	9	} Very great storm of wind and rain.	WNW		28.112	28.59		
	10			90	28.120	28.63	37	
9	6	Fair.	WbyN 40		29. 48	29.25	39	0.82
	12	Showers.			29. 60	29.31	45	
	9				29.132	29.09	47	

March 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom. °	Rain. Inch.
					Inches	Inches		
10	6	Showery day.	SE 20		29.144	29.75	43	
	12		SW 25		29.140	29.73	50	
	9		W		29.140	29.73	48	
11	6	Fair.	W 20		29.138	29.72	45	
	12		40		29.96	29.50	53	
	9	Very stormy.	85		29.48	29.25	48	
12	6	Stormy.	NW 75		29.92	29.48	42	
	12	Hail showers.			29.140	29.73	45	
	9	Fair.	80		29.184	29.96	39	
13	6	Fair and frost.	NE 20	W	30.12	30.6	34	
	12				30.20	30.11	46	
	9		WSW 15		30.10	30.5	40	
14	6	Cloudy, frost.	ENE 20		30.8	30.4	34	
	12	Fair.			30.16	30.9	44	
	9				30.16	30.9	37	
15	6	Fair, frost.	WNW 25		30.24	30.13	33	
	12				30.26	30.14	40	
	9	Cloudy.			30.20	30.11	40	
16	6	Fair.	NNE 30		30.17	30.9	39	
	12				30.17	30.9	41	
	9		W		30.10	30.5	40	
17	6	Cloudy.	NNW 35		30.0	30.0	38	0.10
	12	Fair.			29.188	29.98	45	
	9				29.190	29.99	40	
18	6	Hazy.	WNW 10		30.0	30.0	37	
	12	Somewhat fair.			29.186	29.97	45	
	9	Cloudy.			29.180	29.94	41	

March.

March 1782

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts. Inches.	Barom. 100 parts. Inches.	Thermom. °	Rain. Inch.
19	6	Cloudy.	W 25		29.150	29.79	45	
	12	Some small rain.			29.136	29.71	50	
	9				29.120	29.63	42	
20	6	Hazy.	SW 15		29.140	29.73	39	
	12	Fair.			29.152	29.80	45	
	9				29.144	29.75	41	
21	6	{ Sleet, but snow on the distant hills.	SE 45		29.128	29.67	37	
	12				29.96	29.50	35	
	9				29.40	29.21	35	0.40
22	6	Ditto.	E 60		28.160	28.84	33	
	12		ENE		28.170	28.89	34	
	9				29.0	29.0	32	0.50
23	6	Excessive frost, with a little snow here, but five or six feet deep in the country.	E 10 N 30		29.20 29.32 29.88	29.11 29.17 29.46	27 35 37	
24	6	Some showers.	NW 50		29.100	29.52	40	
	12				29.90	29.47	43	
	9				29.100	29.52	40	0.33
25	6	Cloudy.	W 30		29.108	29.57	40	
	12	Fair.			29.112	29.59	47	
	9				29.128	29.67	39	
26	6	Showery.	WNW 25		29.144	29.75	45	
	12				29.140	29.73	50	
	9				29.140	29.73	48	
27	6	Showery.	W 32		29.128	29.67	48	
	12				29.120	29.63	51	0.17
	9	Hard rain all night.	SW 50		29.88	29.46	48	

March

March 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
28	6	Hard rain.	SSW 50		29.32	29.17	51	
	12	Fair and mild.	30		29.90	29.47	55	
	9	Cloudy.			29.72	29.37	50	1.25
29	6	Hard rain.	WSW 60		29.48	29.25	47	
	12	Fair.			29.60	29.31	50	
	9				29.78	29.41	49	
30	6	Fair.	WNW 50		29.32	29.17	48	
	12				29.64	29.34	55	
	9		NW		29.92	29.48	50	
31	6	Very stormy.	W 60		29.38	29.20	47	
	12	Showers.			29.56	29.29	50	
	9		80		29.46	29.24	46	0.34
Total rain								3.91

April

April 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Therm.	Rain.
					Inches.	Inches.		Inch.
1	6	Hard rain showers; the bar. fell till 8 in the evening; after which it rose 1 line by 10; after which the wind rose to a great storm, but no rain.	SE 60		29. 0	29. 0	42	
	12		SSW 50		28.116	28.61		
	8		W 30		28. 96	28.50	50	
	9		WNW		28. 44	28.23		
	10				28. 60	28.31	43	
2	6		NW 90		28.140	28.73	40	
	12				28.156	28.82	41	
	9		70		28.172	28.90	40	0.33
3	6	Some showers.	NW 50		29. 0	29. 0	38	
	12	Fair.			29. 16	29. 9	45	
	9				29. 48	29.25	37	
4	6	Cloudy.	WNW 40		29. 76	29.40	40	
	12	Fair.			29. 80	29.46	45	
	9		10		29. 75	29.39	38	
5	6	Hard hail showers.	E 40		29. 60	29.31	41	
	12	Fair.	NW 25	NEE	29. 72	29.38	41	
	9				29. 80	29.42	45	
6	6	Showers.	WNW 30		29. 96	29.50	40	
	12		E NW		29.108	29.57	44	
	9	Hail showers.	NE 40		29.112	29.59	40	
7	6	Cloudy.	NE 50		29.128	29.67	40	
	12				29.156	29.82	45	
	9				30. 0	30. 0	41	
8	6	Cloudy.	NE 30		30. 0	30. 0	41	
	12	Fair.			30. 0	30. 0	45	
	9				30. 0	30. 0	40	
9	6	Cloudy.	NE 20		29.184	29.96	40	
	12	Fair.			29.186	29.97	44	
	9				29.186	29.97	40	

April

Sept at Minehead, in Somersetshire.

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L

April

April 1782

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
19	6	Fair.	ESE 25		29.112	29.59	40	
	12				29.116	29.61	44	
	9				29.116	29.61	41	
20	6	Showery.	E 15		29.128	29.67	42	
	12				29.128	29.67	45	
	9				29.140	29.73	40	
21	6	Rain.	E 20		29.130	29.68	41	
	12		SW 35		29.120	29.63	46	
	9				29.128	29.67	41	0.10
22	6	Fair.	SW 40		29.115	29.60	50	
	12	A little rain.			29.126	29.66	56	
	9	Fair.	SE 45		29.123	29.64	46	
23	6	Fair.	SW 30		29.110	29.57	46	
	12	Showers.			29.115	29.60	55	
	9				29.107	29.56	49	
24	6	Showers.	SE 25		29. 78	29.41	50	
	12				29. 66	29.34	49	
	9	Hard rain.			29. 51	29.27	50	
25	6	Fair.	W 20		29. 73	29.38	49	0.67
	12		SE	W	29. 96	29.50	50	
	9				29. 96	29.50	49	
26	6	Fair.	E 30		29.112	29.59	48	
	12				29.116	29.61	50	
	9				29.126	29.66	47	
27	6	Cloudy, black easterly wind.	E 55		29.142	29.74	45	
	12				29.142	29.74	49	
	9				29.146	29.76	45	

April

kept at Minehead, in Somersetshire.

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April 1782.

L 2

May

May, 1782.

May

Sept 2: Minehead, in Somersetshire.

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May: 1781.

May

May 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts. Inches.	Barom. 100 parts. Inches.	Thermom. °	Rain. Inch.
19	6	Rain.	NW 20		29.116	29.61	49	
	12	Fair.			29.134	29.70	51	
	9				29.164	29.86	45	
20	6	Fair.	S SE by E 30		29.161	29.84	49	
	12				29.116	29.61	59	
	9	Hard rain.			29. 76	29.40	42	
21	6	Rain.	NE 45 WSW		29. 88	29.46	46	
	12				29.104	29.54	49	
	9	Cloudy.			29.104	29.54	47	
22	6	Foggy, rain.	WNW 20		29.108	29.56	50	
	12	Fair.			29. 96	29.50	55	
	9	Rain in the night.			29. 90	29.47	48	
23	6	Showers.	SSE 40 WNW		29. 75	29.89	47	0.93
	12	Fair.			29. 90	29.47	50	
	9				29. 98	29.51	49	
24	6	Showers.	NW 30 60		29.112	29.59	50	
	12	Fair.			29.136	29.71	55	
	9				29.160	29.84	50	
25	6	Hazy.	SW 30		29.176	29.92	50	
	12	Fair.			29.170	29.89	54	
	9				29.146	29.77	50	
26	6	Fair.	W 30		29.154	29.81	50	0.24
	12				29.173	29.90	56	
	9	Rainy.			29.170	29.89	53	
27	6	Hazy.	SW 25 S		29.144	29.75	48	
	12	Showers.			29.144	29.75	62	
	9				29.140	29.73	55	

May

May 1782.

Day.	Hour.	Weather,	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
28	6	Cloudy.	SW 0		29.140	29.73	60	
	12	Rain.	SE 20		29.134	29.70	60	
	9	Hard rain.	SW		29.106	29.55	62	
29	6	Rain.	S 30		29.96	29.50	60	
	12				29.88	29.46	58	
	9	Cloudy.	W		29.90	29.47	52	0.33
30	6	Small showers.	W 25		29.94	29.49	57	
	12	Fair at intervals.			29.106	29.55	62	
	9				29.106	29.55	55	
31	6	Cloudy.	W 0		29.96	29.50	52	
	12	Hard showers.			29.96	29.50	60	
	9				29.96	29.50	58	0.28
Total rain								3.21

June

June 1782.

June

Sept. at Minchard, at Schenck's (Ala.)

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June: 1782.

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M

June

June 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.		Inch.
19	6 12 9	Fair.	WNW 10		30. 16 30. 34 30. 32	30. 9 30. 18 30. 17	58 67 60	
20	6 12 9	Fair.	ENE 10 WSW	W	30. 32 30. 38 30. 38	30. 17 30. 20 30. 20	58 68 64	
21	6 12 9	Hazy. Hot.	NNW 20 SW		30. 44 30. 57 30. 76	30. 23 30. 30 30. 40	60 70 67	
22	6 12 9	Fair and hot.	W 25 SE	SW	30. 76 30. 76 30. 76	30. 40 30. 40 30. 40	60 71 60	
23	6 12 9	Fair, very hot.	ENE 30 ESE 40		30. 72 30. 61 30. 48	30. 38 30. 32 30. 25	59 74 66	
24	6 12 9	Fair, very hot.	ESE 20 S by E 10		30. 30 30. 22 30. 19	30. 16 30. 12 30. 10	62 76 67	
25	6 12 2 9	Fair. Excessive hot.	W 0 E 22 S	SW	30. 8 29. 186 29. 184	30. 4 29. 97 29. 96	67 77 80 72	
26	6 12 9	Foggy and cooler. Fair.	WNW 25		29. 184 29. 184 29. 184	29. 96 29. 96 29. 96	61 70 58	
27	6 12 9	Fair.	E 0 WNW 30		30. 10 30. 10 30. 0	30. 5 30. 5 30. 0	57 67 56	

June

Appt at Minthead, in Somersetshire.

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June 1782.

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July

July 1781.

July

Sept at Minehead, in Somersetshire.

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July 1782.

July

July 1782.

July

July 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
28	6	Showers.	W 25		29.140	29.73	60	
	12				29.132	29.69	62	
	9	Cloudy.			29.138	29.72	60	
29	6	Fair.	E 10		29.150	29.79	60	
	12	Hazy.	W 35		29.154	29.81	70	
	9	A little rain.	NW		29.163	29.85	60	
30	6	Fair.	NE 20		29.170	29.89	56	0.77
	12			NE	29.172	29.90	65	
	9		WNW 40		29.174	29.91	58	
31	6	Cloudy.	WNW 15		29.160	29.84	56	
	12	Some rain.		SE	29.156	29.82	62	
	9		SW		29.144	29.75	60	
Total rain								2.93

August

August 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192. parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.		Inch.
1	6 12 9	Flying showers.	NE 30	S	29.140 29.128 29.90	29.73 29.67 29.47	58 68 60	
2	6 12 9	Fair. Showers. Showers.	NE 0 WSW 30 WNW	S	29.70 29.60 29.72	29.37 29.31 29.38	58 66 60	
3	6 12 9	Fair.	NW 40 WNW		29.75 29.110 29.113	29.39 29.58 29.60	60 67 60	
4	6 12 9	Fair. Showers.	WNW 35		29.122 29.134 29.144	29.64 29.70 29.75	58 64 60	
5	6 12 9	Some rain. Fair.	W 10		29.176 29.168 29.160	29.92 29.88 29.84	57 65 59	0.19
6	6 12 9	Showers. Fair. Thunder showers.	W SW S		29.126 29.96 29.48	29.66 29.50 29.25	58 68 60	0.11
7	6 12 9	Hard rain. Very stormy.	ESE 20 NW 90		28.186 29.19 29.39	28.97 29.10 29.20	57 51 55	1.51
8	6 12 9	Very stormy showers. Stormy, but fair.	NNW 90 80 75		29.56 29.100 29.125	29.29 29.52 29.65	56 64 60	0.30
9	6 12 9	Cloudy. Showers.	NW 40		29.120 29.116 29.122	29.63 29.61 29.64	58 62 60	

August 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.		Inch.
10	6	Small showers.	NW 50		29.135	29.71	58	
	12	Fair at intervals.			29.140	29.73	60	
	9				29.154	29.81	57	
11	6	Fair.	WNW 35		29.160	29.84	58	
	12				29.160	29.84	61	
	9		10		29.156	29.82	52	
12	6	Cloudy.	SE 30		29.134	29.70	58	
	12	Hard rain.			29.105	29.54	55	
	9				29.100	29.52	52	
13	6	Fair.	WSW 10		29.100	29.52	57	0.54
	12	Flying showers.			29. 76	29.40	64	
	9	Hard wind and rain.	75		29. 36	29.19	60	
14	6	Hard showers.	SW 75		29. 34	29.18	63	
	12	Fair.			29. 30	29.16	68	
	9	Cloudy.			29. 24	29.13	60	
15	6	Cloudy.	WNW 0		29. 80	29.42	62	
	12	Fair.			29.100	29.52	68	
	9				29.116	29.61	60	0.23
16	6	Rain.	S		29. 48	29.25	56	
	12	Fair.	WNW 20		29. 60	29.31	64	
	9	Cloudy.			29. 86	29.45	59	
17	6	Cloudy.	WNW 30		29. 86	29.45	58	
	12	Fair.			29. 86	29.45	60	
	9	Cloudy.			29. 86	29.45	59	
18	6	Fair.	NW 35		29.138	29.72	60	
	12		WNW		29.140	29.73	64	
	9	Showers.			29.144	29.75	60	

August 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts. Inches.	Barom. 100 parts. Inches.	Thermom. °	Rain. Inch.
19	6	Fair.	SE 10		29.134	29.70	59	0.17
	12				29.146	29.76	69	
	9	Cloudy.	NW		29.148	29.77	60	
20	6	Cloudy.	SW 40		29.148	29.77	58	
	12	Showers.	WNW		29.144	29.75	70	
	9				29.144	29.75	60	
21	6	Fair.	WSW 25		29.152	29.79	58	
	12		SW		29.148	29.77	71	
	9	Showers.			29.154	29.80	63	
22	6	Showery day.	SW 40		29.144	29.75	58	
	12		WSW		29.134	29.70	67	
	9				29.122	29.64	57	
23	6	Showers.	W 50		29.126	29.66	56	
	12	Fair.	NW		29.138	29.73	67	
	9	Cloudy.	SW		29.154	29.81	60	
24	6	Cloudy.	SW 20		29.150	29.79	58	
	12	Showers.			29.115	29.60	70	
	9		WNW		29. 96	29.50	60	
25	6	Fair.	WSW 25		29.106	29.56	59	
	12				29.134	29.70	67	
	9	Showers.	W		29.150	29.79	60	
26	6	Showers:	WSW 20		29.154	29.81	60	
	12				29.163	29.85	70	
	9				29.173	29.90	61	0.64
27	6	Fair.	NW 45		29.138	29.72	60	
	12				29.135	29.70	64	
	9	Hard rain.	WNW		29.106	29.55	60	

August

August 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.		Inch.
28	6	Fair.	NW 50		29.122	29.64	58	
	12	Cloudy.			29.134	29.70	63	
	9	Showers.			29.130	29.68	58	
29	6	Showers.	W 40 WNW 65		29.111	29.58	52	
	12	Fair.			29.118	29.62	59	
	9	Hard rain.			29.123	29.64	54	
30	6	Showers.	NWbyW 45		29.142	29.74	55	
	12	Fair.			29.156	29.81	59	
	9				29.182	29.95	52	
31	6	Showers.	NWbyW 30		30. 4	30. 2	52	0.50
	12	Fair.			30. 16	30. 9	60	
	9	Rain.			30. 24	30.13	55	
Total rain								4.19

N 2

September

September 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
1	6	Cloudy.	WSW 20		30. 32	30.17	57	
	12	Fair.	WNW		30. 37	30.20	60	
	9	Cloudy.			30. 40	30.21	54	0.17
2	6	Cloudy.	SW 10		30. 52	30.27	60	
	12	Fair.	NE NW		30. 66	30.34	68	
	9		SW		30. 48	30.25	58	
3	6	Cloudy.	SW 5		30. 29	30.15	52	
	12		NE SE	SW	30. 20	30.11	67	
	9	Fair.	SW		30. 5	30. 3	60	
4	6	Fair.	NNE 10	SSE	29.180	29.94	55	
	12	Hot.	SSE		29.180	29.94	71	
	9				29.184	29.96	60	
5	6	Fair, cold rain.	SE 20		30. 0	30. 0	52	
	12	Hot.			30. 5	30. 3	70	
	9				30. 10	30. 5	60	
6	6	Fair.	SE 25		30. 10	30. 5	55	
	12				30. 15	30. 8	68	
	9				30. 15	30. 8	60	
7	6	Fair.	SSE 15		30. 18	30.10	60	
	12				30. 18	30.10	70	
	9				30. 20	30.11	60	
8	6	Fair.	E 10		30. 24	30.13	57	
	12				30. 24	30.13	67	
	9				30. 20	30.11	60	
9	6	Foggy.	E 10		30. 24	30.13	60	
	12	Fair.			30. 24	30.13	68	
	9				30. 14	30. 8	62	

September

September 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.		Inch.
10	6	Foggy.	E 15		30. 16	30. 9	58	
	12	Fair.			30. 10	30. 5	67	
	9				30. 0	30. 0	61	
11	6	Fair.	ENE 30		29. 184	29. 96	60	
	12				29. 180	29. 94	67	
	9		50		29. 178	29. 93	60	
12	6	Cloudy.	E 50		29. 179	29. 93	63	
	12				30. 0	30. 0	66	
	9	Fair.			30. 12	30. 6	60	
13	6	Cloudy.	E 20		30. 12	30. 6	55	
	12	Fair.			30. 0	30. 0	65	
	9		W		29. 184	29. 96	58	
14	6	Cloudy.	E 25		29. 170	29. 89	57	
	12				29. 165	29. 87	64	
	9				29. 144	29. 75	60	
15	6	A little rain.	S 25		29. 96	29. 50	58	
	12	Fair.	ESE		29. 64	29. 34	63	
	9				29. 48	29. 25	60	
16	6	A little rain.	S 10		29. 20	29. 11	60	
	12	Hard rain.	ESE 15		29. 12	29. 7	67	
	4				29. 0	29. 0		
	9	Stormy wind and rain.	SW 75		29. 33	29. 17	57	
17	6	Fair.	W 25		29. 48	29. 25	55	
	12	Showers.			29. 57	29. 30	63	
	9				29. 57	29. 30	5	0.61
18	6	Showers.	WNW 30		29. 48	29. 25	55	
	12				29. 20	29. 11	53	
	9		70		29. 57	29. 30	58	

September.

September 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
19	6	Fair and cold.	WNW 60		29.130	29.68	51	
	12				29.170	29.89	53	
	9				29.176	29.92	52	
20	6	Showers.	WNW 30		29.150	29.79	60	
	12				29.142	29.74	64	
	9				29.110	29.58	60	0.22
21	6	Cloudy.	SW 25		29.100	29.52	58	
	12	Hard rain.			29. 96	29.50	60	
	9				29. 96	29.50	61	
22	6	Fair.	SW 28		29.100	29.52	60	
	12	Hard showers.			29.100	29.52	67	
	9				29. 90	29.47	61	0.24
23	6	Showers.	NW 60		29. 76	29.40	57	
	12				29. 80	29.42	64	
	9	Stormy.	70		29.140	29.73	60	
24	6	A little shower.	WNW 10		29.164	29.86	53	
	12	Cloudy.			29.180	29.94	63	
	9	Rain.			29.170	29.89	58	0.88
25	6	Showers.	WNW 5		29.156	29.82	53	
	12	Fair and hot.			29.164	29.80	69	
	9				29.164	29.86	60	
26	6	Showers.	W 0		29.160	29.84	55	
	12	Fair and hot.			29.160	29.84	70	
	9	Cloudy.	20		29.160	29.84	58	0.45
27	6	Rain.	NW 30		29.125	29.65	55	
	12				29.150	29.79	67	
	9	Fair.			29.182	29.95	60	

September

September 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
28	6	Cloudy and cold.	SW 10		30. 6	30. 3	49	
	12		35		30. 12	30. 6	57	
	9	Rain.			29.180	29.94	55	
29	6	Fair.	W 20		29.170	29.89	60	
	12	Showers.			29.140	29.73	67	
	9	Hard rain.			29.128	29.67	61	
30	6	Fair.	NW 60		29.116	29.61	50	
	12	Showers.			29.144	29.75	55	
	9				29.180	29.94	51	
								1.33
Total rain								3.90

October:

October 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom. T	Rain.
					Inches.	Inches.	°	Inch.
1	7	Fair.	NW 30		30. 8	30. 4	49	
	12				30. 10	30. 5	54	
	9				30. 0	30. 0	50	
2	7	Showers.	WNW 35		29. 80	29. 42	55	
	12				29. 70	29. 37	64	
	9				29. 48	29. 25	57	0.41
3	7	Fair.	NE 15		29. 30	29. 16	58	
	12	Cloudy.			29. 60	29. 31	63	
	9				29. 80	29. 42	56	
4	7	Fair and cold.	NE 20		29. 88	29. 45	43	
	12				29. 90	29. 47	53	
	9	Cloudy.			30. 12	30. 6	50	
5	7	Fair.	ENE 10		29. 172	29. 89	45	
	12				29. 180	29. 94	48	
	9				29. 186	29. 97	48	
6	7	Fair.	E 25		30. 6	30. 3	50	
	12				30. 0	30. 0	56	
	9	Cloudy.			29. 184	29. 96	51	
7	7	Cloudy.	WNW 10		29. 175	29. 91	50	
	12	A little rain.			29. 175	29. 91	55	
	9				29. 170	29. 89	51	
8	7	Moist close day.	NW 30		29. 164	29. 86	53	
	12				29. 160	29. 84	56	
	9				29. 160	29. 84	51	
9	7	Some rain.	ESE 24		29. 154	29. 81	53	
	12				29. 140	29. 73	57	
	9				29. 136	29. 71	54	

October

October 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
10	7	Cloudy.	E 30		29. 80	29.42	50	
	12				29. 40	29.21	56	
	9		60		29. 16	29. 9	52	
11	7	Cloudy.	E 60		29. 16	29. 9	49	
	12				29. 40	29.21	54	
	9				29. 60	29.31	51	
12	7	Cloudy.	E 50		29. 84	29.44	50	
	12				29. 88	29.46	54	
	9				29.110	29.57	51	
13	7	Fair.	E 20		29.130	29.68	48	
	12				29.154	29.81	52	
	9	Cloudy.			29.162	29.85	50	
14	7	White frost.	W 10		29.184	29.96	37	
	12				30. 0	30. 0	47	
	9		E		30. 6	30. 3	50	
15	7	Some rain.	E 20		30. 16	30. 9	50	
	12				30. 16	30. 9	54	
	9				30. 16	30. 9	51	
16	7	Cloudy.	WNW 30		30. 16	30. 9	47	
	12	Fair.			30. 18	30.10	55	
	9				30. 20	30.11	50	
17	7	Fair.	N 35		30. 24	30.13	49	
	12		NW		30. 28	30.15	55	
	9	Cloudy.			30. 36	30.19	51	
18	7	Cloudy.	WNW 50		30. 58	30.31	50	
	12	Fair.			30. 72	30.38	54	
	9	Cloudy.	70		30. 66	30.34	50	

October 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
19	7	Some little rain.	WNW 60		29.154	29.81	52	
	12				29.160	29.84	56	
	9	Stormy wind.	80		29.165	29.86	50	0.42
20	7	Fair.	NW by N 60		29.170	29.89	47	
	12				29.172	29.90	54	
	9	Rain.			29.170	29.89	55	
21	7	Rain.	NW 40		29.124	29.65	56	
	12	Fair.			29.144	29.75	60	
	9	Cloudy.			29.144	29.75	60	
22	7	Cloudy.	WNW 35		29.144	29.75	60	
	12				29.130	29.68	62	
	9	Rainy night.			29.110	29.58	62	
23	7	Fair.	WNW 20		29.144	29.75	48	
	12				29.160	29.84	53	
	9				30. 15	30. 8	50	
24	7	Fair, frost.	WNW 0		30. 38	30.20	39	
	12				30. 48	30.25	52	
	9		35		30. 58	30.30	50	
25	7	Showers.	W 20		30. 40	30.21	47	
	12				30. 32	30.17	54	
	9				30. 19	30.10	51	0.40
26	7	Fair.	W 0		30. 16	30. 9	55	
	12				30. 40	30.21	60	
	9	Foggy.			30. 60	30.31	55	
27	7	Foggy day.	SW 0		30. 56	30.29	54	
	12				30. 48	30.25	58	
	9				30. 32	30.17	57	

October 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 180 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
28	7	Fair.	W 5		29.184	29.96	50	
	12	Small showers.	25		29.176	29.92	55	
	9	Stormy showers.	75		29.104	29.54	54	
29	7	Excessive stormy, but dry.	WNW 90		29.136	29.72	50	
	12		NW 80		29.160	29.84	55	
	9	Shower.	10		30. 8	30. 4	51	
30	7	Rain.	WNW 0		30. 0	30. 0	40	
	12				29.140	29.73	52	
	9		30		29. 96	29.50	50	
31	7	Showers.	NW 25		29. 68	29.36.	48	
	12				29. 79	29.42	50	
	9				30. 0	30. 0	46	0.46
Total rain								1.69

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November

November 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
1	8	Cloudy.	SE 15		30. 0	30. 0	42	
	12	Showers.			29. 97	29.50	49	
	9				30. 6	30. 3	45	
2	8	Rain.	SE 30		29.170	29.89	45	
	12				29. 84	29.44	50	
	9				29. 76	29.40	43	
3	8	Fair.	NW 10		29. 6	29. 3	45	
	12	Showers.			29. 12	29. 6	49	
	9				29. 24	29.13	49	
4	8	Fair.	NW 30		29. 84	29.44	40	0.73
	12				29. 90	29.47	45	
	9				29. 96	29.50	44	
5	8	Rain.	NE 35		29.104	29.54	39	
	12				29.136	29.71	43	
	9				29.136	29.71	43	
6	8	Fair, frost.	NW 23		30. 0	30. 0	38	
	12	Snow on the hills.			30. 12	30. 6	43	
	9				30. 50	30.26	37	
7	8	Fair, hard frost.	NNW 0	E	30. 56	30.30	32	
	12				30. 64	30.33	38	
	9				30. 56	30.30	32	
8	8	Fair, hard frost.	NE 20		30. 32	30.17	30	
	12				30. 24	30.13	40	
	9	Cloudy.			30. 16	30. 9	38	
9	8	Cloudy, little frost.	NW 20		30. 0	30. 0	37	
	12	Frost.			30. 0	30. 0	43	
	9				29.178	29.93	32	

November

November 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
10	8	Very hard frost.	NW 0		29.170	29.89	27	
	12				29.166	29.87	35	
	9				29.164	29.86	38	
11	8	Rain.	ENE 25		29.144	29.75	37	
	12				29.160	29.84	42	
	9				29.172	29.90	40	
12	8	Foggy rain.	SSE 30		30. 0	30. 0	39	
	12				30. 8	30. 4	45	
	9	Fair.			30. 20	30.11	48	0.12
13	8	Fair, frost.	NW 0		30. 80	30.42	32	
	12	N. B. The tide ebbed and flowed 3 times in an hour.	SE		30. 96	30.50	45	
	9		W		30.120	30.63	35	
14	8	Fair, frost.	W 0		30.114	30.60	35	
	12	Cloudy.			30.112	30.59	40	
	9				30.110	30.58	40	
15	8	Cloudy.	NW 10		30.108	30.57	48	
	12	Rain.	50		30. 90	30.47	53	
	9				30. 48	30.25	50	
16	8	Cloudy.	NW 75		30. 0	30. 0	48	
	12	Fair.			30. 0	30. 0	52	
	9				30. 16	30. 9	45	
17	8	Fair, little frost.	NNW 30		30. 38	30.20	42	
	12				30. 32	30.17	45	
	9				30. 30	30.16	40	
18	8	Cloudy.	W 10		29.176	29.92	44	
	12	Small rain.			29.174	29.91	48	
	9				29.172	29.90	40	

November,

November 1782.

Day.	Hour.	Weather:	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom. °.	Rain. Inch.
					Inches.	Inches.		
19	8	Foggy, rain.	NW 20		29.180	29.94	40	
	12				29.180	29.94	47	
	9				29.180	29.94	43	
20	8	Cloudy.	E 15		29.180	29.94	40	0.10
	12	Fair.			29.180	29.94	47	
	9				29.180	29.94	41	
21	8	Fair, hard frost.	WNW 0		29.180	29.94	31	
	12				30. 8	30. 4	37	
	9				30. 0	30. 0	30	
22	8	Very hard frost.	S 0		29.154	29.81	29	
	12		SE		29.128	29.67	39	
	9	Cloudy.	W		29.104	29.54	38	
23	8	Cloudy.	NW 10		29. 80	29.42	38	
	12				29. 72	29.38	40	
	9	Snow.	SE		29. 70	29.37	36	
24	8	Cloudy.	S by E 15		29. 66	29.34	35	
	12		E		29. 56	29.29	39	
	9	Rain.			29. 62	29.32	38	
25	8	Hard rain.	S by E 10		29. 90	29.47	37	
	12				29. 96	29.50	38	
	9	Cloudy.			29.130	29.68	38	
26	8	Fair.	S		29.180	29.94	37	
	12				30. 0	30. 0	40	
	9	Freezing hard.	S by E 20		30. 8	30. 4	32	
27	8	Cloudy.	S by E 20		29.160	29.84	38	
	12	Rain.	SW		29.152	29.80	40	
	9				29.144	29.75	40	

November

November 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
28	8	Rain.	S by E 30		29.136	29.71	39	
	12		SSW		29.130	29.68	42	
	9	Cloudy.			29.112	29.59	42	0.54
29	8	Hail showers.	NW 70		29.134	29.70	40	
	12				29.134	29.70	43	
	9		E		29.134	29.70	42	
30	8	Rain.	E 25		29.120	29.63	40	
	12	Fair.			29.126	29.66	43	
	9				29.130	29.68	40	
Total rain								1.49

December

December 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 19.2 parts.	Barom. 100 parts	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
1	8	Cloudy.	ENE 10		29.154	29.81	37	
	12				29.160	29.84	42	
	9	Fair.			29.173	29.90	39	
2	8	Excessive hard frost.	W 0		29.153	29.80	26	
	12		SE	W	29.153	29.80	35	
	9		ESE		29.153	29.80	30	
3	8	Fair, frost.	SSE 10		30. 0	30. 0	32	
	12				30. 0	30. 0	35	
	9	Cloudy.			30. 0	30. 0	32	
4	8	Cloudy, frost.	E by S 15		30. 20	30.11	32	
	12				30. 20	30.11	34	
	9				30. 20	30.11	32	
5	8	Cloudy, frost.	E by S 25		29.176	29.92	34	
	12				29.168	29.88	34	
	9				29.154	29.81	34	
6	8	Cloudy, frost.	S by W 10		29.134	29.70	40	
	12				29.126	29.65	45	
	9	Misling rain.	S		29.126	29.66	42	
7	8	Cloudy.	S by W 12		29.140	29.73	41	
	12	Some rain.			29.140	29.73	49	
	9				29.152	29.80	47	0.14
8	8	Foggy and cold.	E 15		30. 0	30. 0	35	
	12				30. 8	30. 4	35	
	9				30. 8	30. 4	35	
9	8	Foggy.	E 20		29.154	29.81	35	
	12				29.150	29.79	37	
	9				29.150	29.79	37	

kept at Minehead, in Somersetshire.

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December 1782.

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December

December 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
19	8	Frosty and fair.	W	0	30.92	30.48	40	
	12			10	30.116	30.61	45	
	9	Hazy.			30.128	30.67	41	
20	8	Foggy.	W	15	30.120	30.63	39	
	12				30.120	30.63	42	
	9				30.116	30.61	40	
21	8	Cloudy.	WNW	20	30.116	30.61	39	
	12	Fair.			30.120	30.63	48	
	9		NW		30.116	30.61	41	
22	8	Frosty and fair.	E	0	30.112	30.59	39	
	12				30.106	30.55	44	
	9	Cloudy.			30.106	30.55	46	
23	8	Fair.	SW	20	30.72	30.38	40	
	12	Cloudy.	WNW		30.70	30.37	45	
	9				30.57	30.30	40	
24	8	Cloudy.	NW	75	30.50	30.26	45	
	12				30.50	30.26	48	
	9				30.50	30.26	45	
25	8	Foggy.	NW	25	30.28	30.15	45	
	12				30.40	30.21	49	
	9				30.64	30.33	40	
26	8	Foggy.	WNW	15	30.96	30.50	44	
	12				30.96	30.50	48	
	9				30.96	30.50	40	
27	8	Foggy.	WNW	25	30.64	30.34	41	
	12				30.60	30.32	45	
	9				30.60	30.32	39	

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kept at Minehead, in Somersetshire.

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IX. *Description of a Meteor, observed Aug. 18, 1783.**By Mr. Tiberius Cavallo, F. R. S.*

Read Jan. 15, 1784.

BEING upon the Castle Terrace at Windsor, in company with my friend Dr. JAMES LIND, Dr. LOCKMAN, Mr. T. SANDBY, and a few other persons, we observed a very extraordinary meteor in the sky, such as none of us remembered to have seen before. We stood upon the north-east corner of the terrace, where we had a perfect view of the whole phænomenon; and as every one of the company remarked some particular circumstance, the collection of all which furnished the materials for this account; it may be presumed, that this description is as true as the nature of the subject can admit of.

The weather was calm, agreeably warm, and the sky was serene, excepting very near the horizon, where an haziness just prevented the appearance of the stars. A narrow, ragged, and oblong cloud stood on the north-west side of the heavens, reaching from the extremity of the haziness, which rose as high as 18 or 20 degrees, and stretching itself for several degrees towards the east, in a direction nearly parallel to the horizon. It was a little below this cloud, and consequently in the hazy part of the atmosphere, about the N. by W. $\frac{1}{2}$ W. point of the compass,

compass, that this luminous meteor was first perceived. Some flashes of lambent light, much like the *aurora borealis*, were first observed on the northern part of the heavens, which were soon perceived to proceed from a roundish luminous body, nearly as big as the semidiameter of the moon, and almost stationary in the abovementioned point of the heavens (see A in the annexed figure, tab. IV). It was then about 25 minutes after nine o'clock in the evening *. This ball, at the beginning, appeared of a faint bluish light, perhaps from its being just kindled, or from its appearing through the haziness; but it gradually increased its light, and soon began to move, at first ascending above the horizon in an oblique direction towards the east. Its course in this direction was very short, perhaps of five or six degrees; after which it turned itself towards the east, and moving in a direction nearly parallel to the horizon, reached as far as the S. E. by E. where it finally disappeared. The whole duration of the meteor was half a minute, or rather less; and the altitude of its track seemed to be about 25 degrees above the horizon. A short time after the beginning of its motion, the luminous body passed behind the above-mentioned small cloud, so that during this passage we observed only the light that was cast in the heavens from behind the cloud, without actually seeing the body from which it proceeded, for about the sixth or at most the fifth part of its track; but as soon as the meteor emerged from behind the cloud, its light was prodigious. Every object appeared very distinct; the whole face of the country in that beautiful prospect before the terrace

* Mr. SANDBY's watch was seventeen minutes past nine nearest; it does not mark seconds.

being

being instantly illuminated. At this moment the body of the meteor appeared of an oblong form, like that represented at B in the figure; but it presently acquired a tail, and soon after it parted into several small bodies, each having a tail, and all moving in the same direction, at a small distance from each other, and very little behind the principal body, the size of which was gradually reduced after the division (see D in the figure). In this form the whole meteor moved as far as the S. E. by E. where the light decreasing rather abruptly, the whole disappeared.

During the phenomenon no noise was heard by any of our company, excepting one person, who imagined to have heard a crackling noise, something like that which is produced by small wood when burning. But about ten minutes after the disappearance of the meteor, and when we were just going to retire from the terrace, we heard a rumbling noise, as if it were of thunder at a great distance, which, to all probability, was the report of the meteor's explosion; and it may be naturally imagined that this explosion happened when the meteor parted into small bodies, *viz.* at about the middle of its track.

Now if that noise was really the report of the explosion which happened in the abovementioned place, the distance, altitude, course, and other particulars relating to this meteor, must be very nearly as expressed in the following list; they being calculated with mathematical accuracy, upon the preceding particulars; and upon the supposition that sound travels 1150 feet per second. But if the noise we heard was not that of the meteor's explosion, then the following calculations must be considered as quite useless and erroneous.

1

Distance

Meteor observed Aug. 18, 1783.

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Distance of the meteor from Windsor Castle	130 miles.
Length of the path it described in the heavens	550 miles.
Diameter of the luminous body when it came out of the clouds	1070 yards.
Its height above the surface of the Earth	56½ miles.
The explosion must have happened perpendicularly over Lincolnshire.	

T. CAVALLO.

X. An

X. An Account of the Meteors of the 18th of August and 4th of October, 1783. By Alex. Aubert, Esq. F. R. S. and S. A.

Read Jan. 15, 1784.

HAVING been fortunate enough to see both the Meteors, of the 18th of August and of the 4th of October last, I think it my duty to communicate the observations I made upon them to the Royal Society. We are in general so little acquainted with these phænomena, that too many accounts of them cannot be collected, in order to enable us to form some idea of their nature, path, magnitude, and distance from the earth. It is not to be expected, that an observer, in the open air, to whom the appearance comes totally unexpected, can give a perfect account of it; but by going afterwards to the spot from which he saw it, he may, by the assistance of the objects about him, and some proper instruments, come near the truth: I have followed this method; and it is the result thence deduced I have the honour of communicating to the Society.

Monday the 18th of August had been a very sultry day. At the time the meteor made its appearance, although the stars were bright in the upper part of the heavens, the horizon was surrounded with a haziness which did not permit any stars to be seen under an altitude of about eight degrees. I was on horseback, returning to my Observatory at Loampit-hill, near Deptford, in Kent; my face was turned towards the South
West,



West. I was at the foot of Lewisham-bridge, when I was much surpris'd at perceiving suddenly a kind of glimmering light, resembling faint but quickly repeated flashes of lightning; soon after which the light increased much towards the North West; I turned directly to it, and saw it form into a large luminous body like electrical fire, with a tinge of blue round its edges. It rose from the hazy part of the atmosphere (which I have observed might be about 8° high), and moved at first almost in a vertical direction, changing its size and figure continually, having to me all the appearances of successive inflammation, and not of a solid body; it was sometimes round, at others oval and oblong, with its longest diameter in the line of its motion; although it had got high enough to be quite out of the hazy part of the horizon, it was surrounded and accompanied in its whole course with a kind of whitish mist or light vapour. The place from which it rose was about 38° from the north towards the west. After rising a little way perpendicularly, it made its progress in a curve, so as to be at the highest when it had reached due east, at an altitude of about 35° ; after which, continuing a few degrees beyond the east, and being about 30° high, it left behind it several globules of various shapes; the first which detached itself being very small, and the others gradually larger and larger, until the last was nearly as large as the remaining preceding body; soon afterwards they all extinguished gradually, like the bright stars of a sky-rocket, with some inclination downwards, which appearance might probably arise from the upper parts of the separate bodies extinguishing before the lower ones. The meteor was at the brightest and at the largest just before its separation; I estimated its magnitude or area then to be equivalent to two full moons. Its light, during its whole course, was so great,

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that

that I could see every object distinctly, and when it was extinguished the night appeared very dark : I could however see by my watch that it was seventeen minutes after nine : as soon as I got to my observatory, which might be about ten minutes afterwards, having compared it with my regulator, I found it about half a minute too slow for mean time. I think the whole appearance of the meteor, from its first rising out of the hazy part of the atmosphere to its total extinction, did not exceed ten or twelve seconds of time, during which it moved a space corresponding to about 136° in azimuth. I recollect an appearance during its motion, which confirms me in the idea I had of its not being a solid body. In its progress it did not describe a curve as regular as might have been expected from such a body ; but seemed to move in somewhat of a waving line. This irregularity in its course was probably owing to changes of its figure and size, occasioned by the train of inflammation not running in an even line. I should also mention that the meteor appeared extremely near to me, more particularly when it was at the highest ; yet from the comparisons made already of observations at several distant places, we may reasonably judge, that it could not be at less than 40 or 50 miles distance from the surface of the earth.

The meteor of Saturday the 4th of October last was of a much shorter duration and path. I was on horseback, near the stones end, in Blackman-Street, Southwark ; my face was turned northward. I saw, towards the N. N. E. a train of fire, resembling in its motion a common meteor, vulgarly called a falling star, but the colour of it was red ; it originated at an altitude of about 25° , and moved quickly in a strait line eastward, inclining gradually towards the horizon, so as to be, after a course of 15° or 20° in azimuth, about 15° above the horizon,

horizon, when it spread into a broader train, and growing of a lighter colour, it terminated by resolving itself into a beautiful oblong body of the brightest fire, like electrical fire tinged blue, almost as large as the moon; it illuminated the street and houses much more than any lightning I have seen; those who had not a direct view of it, took it for a long flash of lightning. I think its whole course did not exceed 25° , nor the time of its appearance two or three seconds. It extinguished quickly, and left behind it, in its path, a train of very dull reddish fire, which continued visible to my naked eye above one minute and a half. The time of night was forty-three minutes past six; it was a fine star-light evening, warmer than the preceding ones; the moon beyond the first quarter, and very bright; yet her light was not to be compared to the much greater light of the meteor.

I do not recollect hearing any noise or report, either during or after the appearance of these meteors.

London,
Nov. 6, 1783.

ALEXANDER AUBERT.

Since I wrote the above account, I have reason to think I have estimated the altitude of the last meteor rather too low; some of my friends in London, who had, at the time of its appearance, a very good object of comparison for its altitude, make it nearer 30° than 20° degrees.



XI. *Observations on a remarkable Meteor seen on the 18th of August, 1783, communicated in a Letter to Sir Joseph Banks, Bart. P. R. S. By William Cooper, D. D. F. R. S. Archdeacon of York.*

Read Jan. 15, 1784.

DEAR SIR,

Hartlepool, near Stockton,
Aug. 19, 1783.

NO person could have a better opportunity of discerning this awful meteor than myself. The weather being, for this climate, astonishingly hot, my FAHRENHEIT's thermometer, on a north position, and in the open air, having for several days preceding graduated between the hours of ten o'clock in the morning and seven o'clock in the evening from 74° to 82° , I set out upon a journey to the sea-side. The weather was sultry, the atmosphere hazy, and not a breath of air stirring. Towards nine o'clock at night it was so dark, that I could scarcely discern the hedges, road, or even the horses heads. As we proceeded, I observed to my attendants, that there was something singularly striking in the appearance of the night, not merely from its stillness and darkness, but from the sulphureous vapours which seemed to surround us on every side. In the midst of this gloom, and on an instant, a brilliant tremulous light appeared to the N. W. by N. At the first it seemed stationary; but in a small space of time it burst from its position, and took its course to the S. E. by E. It passed directly

Dr. COOPER's Observations on the Meteor of Aug. 18, 1783. 117
rectly over our heads with a buzzing noise, seemingly at the height of sixty yards. Its tail, as far as the eye could form any judgement, was about eight or ten yards in length. At last, this wonderful meteor divided into several glowing parts or balls of fire, the chief part still remaining in its full splendor. Soon after this I heard two great explosions, each equal to the report of a canon carrying a nine-pound ball. During its awful progress, the whole of the atmosphere, as far as I could discern, was perfectly illuminated with the most beautifully vivid light I ever remember to have seen. The horses on which we rode shrunk with fear; and some people whom we met upon the road declared their consternation in the most expressive terms.

I have the honor to be, &c.

WILLIAM COOPER.

XII. *An Account of the Meteor of the 18th of August, 1783. In a Letter from Richard Lovell Edgeworth, Esq. F. R. S. to Sir Joseph Banks, Bart. P. R. S.*

Read Jan. 15, 1784.

DEAR SIR,

Edgeworthstown, Mullingar, Ireland.

AT half past nine in the evening of the 18th of August, I saw the meteor which has been observed in so many different places.

Its size appeared to be about one third of the moon's diameter; and it moved from the north with an equable velocity, at an elevation of ten or twelve degrees, and in a line parallel to the horizon.

It was visible during ten or fifteen seconds, and seemed to be of a parabolic figure, with a luminous tail, twenty or five and twenty of its diameters in length.

It exhibited the most vivid colours; the foremost part being of the brightest blue, followed by different shades of red. Twice during its flight it was eclipsed or extinguished, not gradually, but at once, immersing and emerging with undiminished lustre.

I shall not venture to trouble you with any conjectures upon the nature of this phænomenon, as it is probable, that the subject has been fully discussed long before this time by your friends in London. I am, &c.

Sept. 5, 1783.

RICHARD LOVELL EDGEWORTH.



XIII. *Experiments on Air.* By Henry Cavendish, Esq.
F. R. S. & S. A.

Read Jan. 15, 1784.

THE following experiments were made principally with a view to find out the cause of the diminution which common air is well known to suffer by all the various ways in which it is phlogistified, and to discover what becomes of the air thus lost or condensed; and as they seem not only to determine this point, but also to throw great light on the constitution and manner of production of dephlogistified air, I hope they may be not unworthy the acceptance of this society.

Many gentlemen have supposed that fixed air is either generated or separated from atmospheric air by phlogistification, and that the observed diminution is owing to this cause; my first experiments therefore were made in order to ascertain whether any fixed air is really produced thereby. Now, it must be observed, that as all animal and vegetable substances contain fixed air, and yield it by burning, distillation, or putrefaction, nothing can be concluded from experiments in which the air is phlogistified by them. The only methods I know, which are not liable to objection, are by the calcination of metals, the burning of sulphur or phosphorus, the mixture of nitrous air, and the explosion of inflammable air. Perhaps it may be supposed, that I ought to add to these the electric spark; but I think:

think it much most likely, that the phlogistification of the air, and production of fixed air, in this process, is owing to the burning of some inflammable matter in the apparatus. When the spark is taken from a solution of tournsol, the burning of the tournsol may produce this effect; when it is taken from lime-water, the burning of some foulness adhering to the tube, or perhaps of some inflammable matter contained in the lime, may have the same effect; and when quicksilver or metallic knobs are used, the calcination of them may contribute to the phlogistification of the air, though not to the production of fixed air.

There is no reason to think that any fixed air is produced by the first method of phlogistification. Dr. PRIESTLEY never found lime-water to become turbid by the calcination of metals over it*: Mr. LAVOISIER also found only a very flight and scarce perceptible turbid appearance, without any precipitation, to take place when lime-water was shaken in a glass vessel full of the air in which lead had been calcined; and even this small diminution of transparency in the lime-water might very likely arise, not from fixed air, but only from its being fouled by particles of the calcined metal, which we are told adhered in some places to the glass. This want of turbidity has been attributed to the fixed air uniting to the metallic calx, in preference to the lime; but there is no reason for supposing that the calx contained any fixed air; for I do not know that any one has extracted it from calces prepared in this manner; and though most metallic calces prepared over the fire, or by long exposure to the atmosphere, where they are in contact with fixed air, contain that substance, it by no means follows that they must

* Experiments on Air, vol. I. p. 137.

do so when prepared by methods in which they are not in contact with it.

Dr. PRIESTLEY also observed, that quicksilver, fouled by the addition of lead or tin, deposits a powder by agitation and exposure to the air, which consists in great measure of the calx of the imperfect metal. He found too some powder of this kind to contain fixed air *; but it is by no means clear that this air was produced by the phlogistification of the air in which the quicksilver was shaken; as the powder was not prepared on purpose, but was procured from quicksilver fouled by having been used in various experiments, and may therefore have contained other impurities besides the metallic calces.

I never heard of any fixed air being produced by the burning of sulphur or phosphorus; but it has been asserted, and commonly believed, that lime water is rendered cloudy by a mixture of common and nitrous air; which, if true, would be a convincing proof that on mixing those two substances some fixed air is either generated or separated; I therefore examined this carefully. Now it must be observed, that as common air usually contains a little fixed air, which is no essential part of it, but is easily separated by lime water; and as nitrous air may also contain fixed air, either if the metal from which it is procured be rusty, or if the water of the vessel in which it is caught contain calcareous earth, suspended by fixed air, as most waters do, it is proper first to free both airs from it by previously washing them with lime water †. Now I found, by repeated

* Exper. in Nat. Phil. vol. I. p. 144.

† Though fixed air is absorbed in considerable quantity by water, as I shewed in Phil. Transf. vol. LVI. yet it is not easy to deprive common air of all the fixed

ed experiments, that if the lime water was clean, and the two airs were previously washed with that substance, not the least cloud was produced, either immediately on mixing them, or on suffering them to stand upwards of an hour, though it appeared by the thick clouds which were produced in the lime water, by breathing through it after the experiment was finished, that it was more than sufficient to saturate the acid formed by the decomposition of the nitrous air, and consequently that if any fixed air had been produced, it must have become visible. Once indeed I found a small cloud to be formed on the surface, after the mixture had stood a few minutes. In this experiment the lime water was not quite clean; but whether the cloud was owing to this circumstance, or to the air's having not been properly washed, I cannot pretend to say.

Neither does any fixed air seem to be produced by the explosion of the inflammable air obtained from metals, with either common or dephlogisticated air. This I tried by putting a little lime-water into a glass globe fitted with a brass cock, so as to make it air tight, and an apparatus for firing air by electricity. This globe was exhausted by an air-pump, and the two airs, which had been previously washed with lime-water, let in, and suffered to remain some time, to shew whether they would affect the lime water, and then fired by electricity. The event was, that not the least cloud was produced in the lime-water, when the inflammable air was mixed with common air, and

air contained in it by means of water. On shaking a mixture of ten parts of common air, and one of fixed air, with more than an equal bulk of distilled water, not more than half of the fixed air was absorbed, and on transferring the air into fresh distilled water only half the remainder was absorbed, as appeared by the diminution which it still suffered on adding lime water.

only

only a very slight one, or rather diminution of transparency, when it was combined with dephlogisticated air. This, however, seemed not to be produced by fixed air; as it appeared instantly after the explosion, and did not increase on standing, and was spread uniformly through the liquor; whereas if it had been owing to fixed air, it would have taken up some short time before it appeared, and would have begun first at the surface, as was the case in the abovementioned experiment with nitrous air. What it was really owing to I cannot pretend to say; but if it did proceed from fixed air it would shew that only an excessively minute quantity was produced *. On the whole, though it is not improbable that fixed air may be generated in some chymical processes, yet it seems certain that it is not the general effect of phlogisticating air, and that the diminution of common air is by no means owing to the generation or separation of fixed air from it.

As there seemed great reason to think, from Dr. Priestley's experiments, that the nitrous and vitriolic acids were convertible into dephlogisticated air, I tried whether the dephlogisticated part of common air might not, by phlogistication, be changed into nitrous or vitriolic acid. For this purpose I impregnated some milk of lime with the fumes of burning sulphur, by putting a little of it into a large glass receiver, and burning sulphur therein, taking care to keep the mouth of the receiver stopp'd till the fumes were all absorbed; after which the air of the receiver was changed, and more sulphur burnt in it as before, and the process repeated till 122 grains of sulphur were consumed. The milk of lime was then filtered and evaporated, but it yielded no nitrous salt, nor any other substance except selenite; so that no sensible quantity of the air was changed

* Dr. PRIESTLEY also found no fixed air to be produced by the explosion of inflammable and common air. Vol. V. p. 124.

into nitrous acid. It must be observed, that as the vitriolic acid produced by the burning sulphur is changed by its union with the lime into selenite, which is very little soluble in water, a very small quantity of nitrous salt, or any other substance which is soluble in water, would have been perceived.

I also tried whether any nitrous acid was produced by phlogisticating common air with liver of sulphur; for this purpose I made a solution of flowers of sulphur by boiling it with lime, and put a little of it into a large receiver, and shook it frequently, changing now and then the air, till the yellow colour of the solution was quite gone; a sign that all the sulphur was, by the loss of its phlogiston, turned into vitriolic acid, and united to the lime, or precipitated; the liquor was then filtered and evaporated, but it yielded not the least nitrous salt.

The experiment was repeated in nearly the same manner with dephlogisticated air procured from red precipitate; but not the least nitrous acid was obtained.

It is well known that common selenite is very little soluble in water; whereas that procured in the two last experiments was very soluble, and even crystallized readily, and was intensely bitter; this however appeared to be owing merely to the acid with which it was formed being very much phlogisticated; for on evaporating it to dryness, and exposing it to the air for a few days, it became much less soluble; so that on adding water to it not much dissolved; and by repeating this process once or twice, it seemed to become not more soluble than selenite made in the common manner.

This solubility of the selenite caused some trouble in trying the experiment; for while it continued much soluble it would have been impossible to have distinguished a small mixture of nitrous salt; but by the abovementioned process I was able to distinguish

distinguish as small a proportion as if the selenite had been originally no more soluble than usual.

The nature of the neutral salts made with the phlogisticated vitriolic and nitrous acids has not been much examined by the chymists, though it seems well worth their attention; and it is likely that many besides the foregoing may differ remarkably from those made with the same acids in their common state. Nitre formed with the phlogisticated nitrous acid has been found to differ considerably from common nitre, as well as Sab Polychrest from vitriolated tartar.

In order to try whether any vitriolic acid was produced by the phlogistication of air, I impregnated fifty ounces of distilled water with the fumes produced on mixing fifty-two ounce measures of nitrous air with a quantity of common air sufficient to decompose it. This was done by filling a bottle with some of this water, and inverting it into a basin of the same, and then, by a syphon, letting in as much nitrous air as filled it half-full; after which common air was added slowly by the same syphon, till all the nitrous air was decomposed. When this was done, the distilled water was further impregnated in the same manner till the whole of the abovementioned quantity of nitrous air was employed. This impregnated water, which was very sensibly acid to the taste, was distilled in a glass retort. The first runnings were very acid, and smelt pungent, being nitrous acid much phlogisticated; what came next had no sensible taste or smell; but the last runnings were very acid, and consisted of nitrous acid not phlogisticated. Scarcely any sediment was left behind. These different parcels of distilled liquor were then exactly saturated with salt of tartar, and evaporated; they yielded 87½ grains of nitre, which, as far as I could perceive, was unmixed with vitriolated tartar or any other.

other substance, and consequently no sensible quantity of the common air with which the nitrous air was mixed was turned into vitriolic acid.

It appears, from this experiment, that nitrous air contains as much acid as 2 $\frac{1}{2}$ times its weight of salt-petre; for fifty-two ounce-measures of nitrous air weigh 32 grains; and, as was before said, yield as much acid as is contained in 87 grains of salt-petre; so that the acid in nitrous air is in a remarkably concentrated state; and I believe more than 1 $\frac{1}{2}$ times as much so as the strongest spirit of nitre ever prepared.

Having now mentioned the unsuccessful attempts I made to find out what becomes of the air lost by phlogistication, I proceeded to some experiments, which serve really to explain the matter. In Dr. PRIESTLEY's last volume of experiments is related an experiment of Mr. WARLTYRE's, in which it is said, that, on firing a mixture of common and inflammable air by electricity, in a close copper vessel holding about three pints, and of weight was always perceived, on an average about two grains; though the vessel was stopped in such a manner that no air could escape by the explosion. It is also related, that on repeating the experiment in glass vessels, the inside of the glass, though clean and dry before, immediately became dewy, which confirmed an opinion he had long entertained, that common air deposits its moisture by phlogistication. As the latter experiment seemed likely to throw great light on the subject I had in view, I thought it well worth examining more closely. The first experiment also, if there was no mistake in it, would be very extraordinary and curious; but it did not succeed with me; for though the vessel I used held more than Mr. WARLTYRE's, namely, 124,000 grains of water, and though the experiment

was repeated several times with different proportions of common and inflammable air, I could never perceive a loss of weight of more than one-fifth of a grain, and commonly none at all. It must be observed, however, that though there were some of the experiments in which it seemed to diminish a little in weight, there were none in which it increased.*

In all the experiments, the inside of the glass globe became dewy, as observed by Mr. WARTTIRE; but not the least sooty matter could be perceived. Care was taken in all of them to find how much the air was diminished by the explosion, and to observe its test. The result is as follows: the bulk of the inflammable air being expressed in decimals of the common air,

Common air.	Inflammable air.	Diminution.	Air remaining after the explosion.	Test of this air in the first method.	Standard.
1,241	,686	1,555	,053		
1,055	,642	1,413	,063		
,796	,647	1,959	,066		
,423	,612	,811	,097		
,331	,476	,855	,339		
,206	,294	,912	,048		

In these experiments the inflammable air was procured from zinc, as it was in all my experiments, except where otherwise expressed: but I made two more experiments, to try whether there was any difference between the air from zinc and that from iron, the quantity of inflammable air being the same in both; namely, 0,331 of the common; but I could not find any difference to be depended on between the two kinds of air,

* Dr. PRIESTLEY, I am informed, has since found the experiment not to succeed.

either

either in the diminution which they suffered by the explosion, or the test of the burnt air.

From the fourth experiment it appears, that 423 measures of inflammable air are nearly sufficient to completely phlogistificate 1000 of common air; and that the bulk of the air remaining after the explosion is then very little more than four-fifths of the common air employed; so that as common air cannot be reduced to a much less bulk than that by any method of phlogistification, we may safely conclude, that when they are mixed in this proportion, and exploded, almost all the inflammable air, and about one-fifth part of the common air, lose their elasticity, and are condensed into the dew which lines the glass.

The better to examine the nature of this dew, 500000 grain measures of inflammable air were burnt with about $2\frac{1}{2}$ times that quantity of common air, and the burnt air made to pass through a glass cylinder eight feet long and three-quarters of an inch in diameter, in order to deposit the dew. The two airs were conveyed slowly into this cylinder by separate copper pipes, passing through a brass plate which stopped up the end of the cylinder; and as neither inflammable nor common air can burn by themselves, there was no danger of the flame spreading into the magazines from which they were conveyed. Each of these magazines consisted of a large tin vessel, inverted into another vessel just big enough to receive it. The inner vessel communicated with the copper pipe, and the air was forced out of it by pouring water into the outer vessel; and in order that the quantity of common air expelled should be $2\frac{1}{2}$ times that of the inflammable, the water was let into the outer vessels by two holes in the bottom of the same tin pan, the hole which conveyed the water into that vessel in which

which the common air was confined being $2\frac{1}{2}$ times as big as the other.

In trying the experiment, the magazines being first filled with their respective airs, the glass cylinder was taken off, and water let, by the two holes, into the outer vessels, till the airs began to issue from the ends of the copper pipes; they were then set on fire by a candle, and the cylinder put on again in its place. By this means upwards of 135 grains of water were condensed in the cylinder, which had no taste nor smell, and which left no sensible sediment when evaporated to dryness; neither did it yield any pungent smell during the evaporation; in short, it seemed pure water.

In my first experiment, the cylinder near that part where the air was fired was a little tinged with sooty matter, but very slightly so; and that little seemed to proceed from the putty with which the apparatus was luted, and which was heated by the flame; for in another experiment, in which it was contrived so that the luting should not be much heated, scarce any sooty tinge could be perceived.

By the experiments with the globe it appeared, that when inflammable and common air are exploded in a proper proportion, almost all the inflammable air, and near one-fifth of the common air, lose their elasticity, and are condensed into dew. And by this experiment it appears, that this dew is plain water, and consequently that almost all the inflammable air, and about one-fifth of the common air, are turned into pure water.

In order to examine the nature of the matter condensed on firing a mixture of dephlogisticated and inflammable air, I took a glass globe, holding 8800 grain measures, furnished with a brass cock and an apparatus for firing air by electricity. This globe was well exhausted by an air-pump, and then filled with

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a mixture

a mixture of inflammable and dephlogisticated air, by shutting the cock, fastening a bent glass tube to its mouth, and letting up the end of it into a glass jar inverted into water, and containing a mixture of 19500 grain measures of dephlogisticated air, and 37000 of inflammable; so that, upon opening the cock, some of this mixed air rushed through the bent tube, and filled the globe*. The cock was then shut, and the included air fired by electricity, by which means almost all of it lost its elasticity. The cock was then again opened, so as to let in more of the same air, to supply the place of that destroyed by the explosion, which was again fired, and the operation continued till almost the whole of the mixture was let into the globe and exploded. By this means, though the globe held not more than the sixth part of the mixture, almost the whole of it was exploded therein, without any fresh exhaustion of the globe.

As I was desirous to try the quantity and test of this burnt air, without letting any water into the globe, which would have prevented my examining the nature of the condensed matter, I took a larger globe, furnished also with a stop cock, exhausted it by an air-pump, and screwed it on upon the cock of the former globe; upon which, by opening both cocks, the air rushed out of the smaller globe into the larger, till it became of equal density in both; then, by shutting the cock of the larger globe, unscrewing it again from the former, and opening it under water, I was enabled to find the quantity of the burnt air in it; and consequently, as the proportion which the contents of the two globes bore to each other was

* In order to prevent any water from getting into this tube, while dipped under water to let it up into the glass jar, a bit of wax was stuck upon the end of it, which was rubbed off when raised above the surface of the water.

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known, could tell the quantity of burnt air in the small globe before the communication was made between them. By this means the whole quantity of the burnt air was found to be 2950 grain measures; its standard was 1,85.

The liquor condensed in the globe, in weight about 30 grains, was sensibly acid to the taste, and by saturation with fixed alkali, and evaporation, yielded near two grains of nitre; so that it consisted of water united to a small quantity of nitrous acid. No footy matter was deposited in the globe. The dephlogisticated air used in this experiment was procured from red precipitate, that is, from a solution of quicksilver in spirit of nitre distilled till it acquires a red colour.

As it was suspected, that the acid contained in the condensed liquor was no essential part of the dephlogisticated air, but was owing to some acid vapour which came over in making it and had not been absorbed by the water, the experiment was repeated in the same manner, with some more of the same air, which had been previously washed with water, by keeping it a day or two in a bottle with some water, and shaking it frequently; whereas that used in the preceding experiment had never passed through water, except in preparing it. The condensed liquor was still acid.

The experiment was also repeated with dephlogisticated air, procured from red lead by means of oil of vitriol; the liquor condensed was acid, but by an accident I was prevented from determining the nature of the acid.

I also procured some dephlogisticated air from the leaves of plants, in the manner of Doctors INGENHOUSZ and PRIESTLEY, and exploded it with inflammable air as before; the condensed liquor still continued acid, and of the nitrous kind.

In all these experiments the proportion of inflammable air was such, that the burnt air was not much phlogisticated; and it was observed, that the less phlogisticated it was, the more acid was the condensed liquor. I therefore made another experiment, with some more of the same air from plants, in which the proportion of inflammable air was greater, so that the burnt air was almost completely phlogisticated, its standard being $\frac{1}{16}$. The condensed liquor was then not at all acid, but seemed pure water: so that it appears, that with this kind of dephlogisticated air, the condensed liquor is not at all acid, when the two airs are mixed in such a proportion that the burnt air is almost completely phlogisticated, but is considerably so when it is not much phlogisticated.

In order to see whether the same thing would obtain with air procured from red precipitate, I made two more experiments with that kind of air, the air in both being taken from the same bottle, and the experiment tried in the same manner, except that the proportions of inflammable air were different. In the first, in which the burnt air was almost completely phlogisticated, the condensed liquor was not at all acid. In the second, in which its standard was $\frac{1}{86}$, that is, not much phlogisticated, it was considerably acid; so that with this air, as well as with that from plants, the condensed liquor contains, or is entirely free from, acid, according as the burnt air is less or more phlogisticated; and there can be little doubt but that the same rule obtains with any other kind of dephlogisticated air.

In order to see whether the acid, formed by the explosion of dephlogisticated air obtained by means of the vitriolic acid, would also be of the nitrous kind, I procured some air from turbith mineral, and exploded it with inflammable air, the propor-

proportion being such that the burnt air was not much phlogisticated. The condensed liquor manifested an acidity, which appeared, by saturation with a solution of salt of tartar, to be of the nitrous kind; and it was found, by the addition of some *terra ponderosa salita*, to contain little or no vitriolic acid.

When inflammable air was exploded with common air, in such a proportion that the standard of the burnt air was about $\frac{1}{10}$, the condensed liquor was not in the least acid. There is no difference, however, in this respect between common air, and dephlogisticated air mixed with phlogisticated in such a proportion as to reduce it to the standard of common air; for some dephlogisticated air from red precipitate, being reduced to this standard by the addition of perfectly phlogisticated air, and then exploded with the same proportion of inflammable air as the common air was in the foregoing experiment, the condensed liquor was not in the least acid.

From the foregoing experiments it appears, that when a mixture of inflammable and dephlogisticated air is exploded in such proportion that the burnt air is not much phlogisticated, the condensed liquor contains a little acid, which is always of the nitrous kind, whatever substance the dephlogisticated air is procured from; but if the proportion be such that the burnt air is almost entirely phlogisticated, the condensed liquor is not at all acid, but seems pure water, without any addition whatever; and as, when they are mixed in that proportion, very little air remains after the explosion, almost the whole being condensed, it follows, that almost the whole of the inflammable and dephlogisticated air is converted into pure water. It is not easy, indeed, to determine from these experiments what proportion the burnt air, remaining after the explosions, bore to the dephlogisticated air employed, as neither the

small

small nor the large globe could be perfectly exhausted of air, and there was no saying with exactness what quantity was left in them; but in most of them, after allowing for this uncertainty, the true quantity of burnt air seemed not more than $\frac{1}{17}$ th of the dephlogisticated air employed, or $\frac{1}{16}$ th of the mixture. It seems, however, unnecessary to determine this point exactly, as the quantity is so small, that there can be little doubt but that it proceeds only from the impurities mixed with the dephlogisticated and inflammable air, and consequently that, if those airs could be obtained perfectly pure, the whole would be condensed.

With respect to common air, and dephlogisticated air reduced by the addition of phlogisticated air to the standard of common air, the case is different; as the liquor condensed in exploding them with inflammable air, I believe I may say in any proportion, is not at all acid; perhaps, because if they are mixed in such a proportion as that the burnt air is not much phlogisticated, the explosion is too weak, and not accompanied with sufficient heat.

All the foregoing experiments, on the explosion of inflammable air with common and dephlogisticated airs, except those which relate to the cause of the acid found in the water, were made in the summer of the year 1781, and were mentioned by me to Dr. PRIESTLEY, who in consequence of it made some experiments of the same kind, as he relates in a paper printed in the preceding volume of the Transactions. During the last summer also, a friend of mine gave some account of them to M. LAVOISIER, as well as of the conclusion drawn from them, that dephlogisticated air is only water deprived of phlogiston; but at that time so far was M. LAVOISIER from thinking any such opinion warranted, that, till he was prevailed

vailed upon to repeat the experiment himself, he found some difficulty in believing that nearly the whole of the two airs could be converted into water. It is remarkable, that neither of these gentlemen found any acid in the water produced by the combustion; which might proceed from the latter having burnt the two airs in a different manner from what I did; and from the former having used a different kind of inflammable air, namely, that from charcoal, and perhaps having used a greater proportion of it.

Before I enter into the cause of these phænomena, it will be proper to take notice, that phlogisticated air appears to be nothing else than the nitrous acid united to phlogiston; for when nitre is deflagrated with charcoal, the acid is almost entirely converted into this kind of air. That the acid is entirely converted into air, appears from the common process for making what is called clyffius of nitre; for if the nitre and charcoal are dry, scarce any thing is found in the vessels prepared for condensing the fumes; but if they are moist a little liquor is collected, which is nothing but the water contained in the materials, impregnated with a little volatile alkali, proceeding in all probability from the imperfectly burnt charcoal, and a little fixed alkali, consisting of some of the alkalized nitre carried over by the heat and watery vapours. As far as I can perceive too, at present, the air into which much the greatest part of the acid is converted, differs in no respect from common air phlogisticated. A small part of the acid, however, is turned into nitrous air, and the whole is mixed with a good deal of fixed, and perhaps a little inflammable air, both proceeding from the charcoal.

It is well known, that the nitrous acid is also converted by phlogistication into nitrous air, in which respect there seems a considerable

considerable analogy between that and the vitriolic acid; for the vitriolic acid, when united to a smaller proportion of phlogiston, forms the volatile sulphureous acid and vitriolic acid air, both of which, by exposure to the atmosphere, lose their phlogiston, though not very fast, and are turned back into vitriolic acid; but, when united to a greater proportion of phlogiston, it forms sulphur, which shews no signs of acidity, unless a small degree of affinity to alkalies can be called so, and in which the phlogiston is more strongly adherent, so that it does not fly off when exposed to the air, unless assisted by a heat sufficient to set it on fire. In like manner the nitrous acid, united to a certain quantity of phlogiston, forms nitrous fumes and nitrous air, which readily quit their phlogiston to common air; but when united to a different, in all probability a larger quantity, it forms phlogisticated air, which shews no signs of acidity, and is still less disposed to part with its phlogiston than sulphur.

This being premised, there seem two ways by which the phenomena of the acid found in the condensed liquor may be explained; first, by supposing that dephlogisticated air contains a little nitrous acid which enters into it as one of its component parts, and that this acid, when the inflammable air is in a sufficient proportion, unites to the phlogiston, and is turned into phlogisticated air, but does not when the inflammable air is in too small a proportion; and, secondly, by supposing that there is no nitrous acid mixed with, or entering into the composition of, dephlogisticated air, but that, when this air is in a sufficient proportion, part of the phlogisticated air with which it is debased is, by the strong affinity of phlogiston to dephlogisticated air, deprived of its phlogiston and turned into nitrous acid; whereas, when the dephlogisticated air is not more than sufficient to consume the inflammable air,

none

none then remains to deprive the phlogificated air of its phlogiston, and turn it into acid.

If the latter explanation be true, I think, we must allow that dephlogificated air is in reality nothing but dephlogificated water, or water deprived of its phlogiston; or, in other words, that water consists of dephlogificated air united to phlogiston; and that inflammable air is either pure phlogiston, as Dr. PRIESTLEY and Mr. KIRWAN suppose, or else water united to phlogiston*; since, according to this supposition, these two substances united together form pure water. On the other hand, if the first explanation be true, we must suppose that dephlogificated air consists of water united to a little nitrous acid and deprived of its phlogiston; but still the nitrous acid in it must make only a very small part of the whole,

* Either of these suppositions will agree equally well with the following experiments; but the latter seems to me much the most likely. What principally makes me think so is, that common or dephlogificated air do not absorb phlogiston from inflammable air, unless assisted by a red heat, whereas they absorb the phlogiston of nitrous air, liver of sulphur, and many other substances, without that assistance; and it seems inexplicable, that they should refuse to unite to pure phlogiston, when they are able to extract it from substances to which it has an affinity; that is, that they should overcome the affinity of phlogiston to other substances, and extract it from them, when they will not even unite to it when presented to them. On the other hand, I know no experiment which shews inflammable air to be pure phlogiston rather than an union of it with water, unless it be Dr. PRIESTLEY's experiment of expelling inflammable air from iron by heat alone. I am not sufficiently acquainted with the circumstances of that experiment to argue with certainty about it; but I think it much more likely, that the inflammable air was formed by the union of the phlogiston of the iron filings with the water dispersed among them, or contained in the retort or other vessel in which it was heated; and in all probability this was the cause of the separation of the phlogiston, as iron seems not disposed to part with its phlogiston by heat alone, without being assisted by the air or some other substance.

as it is found, that the phlogisticated air, which it is converted into, is very small in comparison of the dephlogisticated air.

I think the second of these explanations seems much the most likely; as it was found, that the acid in the condensed liquor was of the nitrous kind, not only when the dephlogisticated air was prepared from red precipitate, but also when it was procured from plants or from turbith mineral: and it seems not likely, that air procured from plants, and still less likely that air procured from a solution of mercury in oil of vitriol, should contain any nitrous acid.

Another strong argument in favour of this opinion is, that dephlogisticated air yields no nitrous acid when phlogisticated by liver of sulphur; for if this air contains nitrous acid, and yields it when phlogisticated by explosion with inflammable air, it is very extraordinary that it should not do so when phlogisticated by other means.

But what forms a stronger and, I think, almost decisive argument in favour of this explanation is, that when the dephlogisticated air is very pure, the condensed liquor is made much more strongly acid by mixing the air to be exploded with a little phlogisticated air, as appears by the following experiments.

A mixture of 18500 grain measures of inflammable air with 9750 of dephlogisticated air procured from red precipitate were exploded in the usual manner; after which, a mixture of the same quantities of the same dephlogisticated and inflammable air, with the addition of 2500 of air phlogisticated by iron filings and sulphur, was treated in the same manner. The condensed liquor, in both experiments, was acid, but that in the latter evidently more so, as appeared also by saturating each of them separately with marble powder, and precipitating the

the earth by fixed alkali, the precipitate of the second experiment weighing one-fifth of a grain; and that of the first being several times less. The standard of the burnt air in the first experiment was 1,86, and in the second only 0,9.

It must be observed, that all circumstances were the same in these two experiments, except that in the latter the air to be exploded was mixed with some phlogisticated air, and that in consequence the burnt air was more phlogisticated than in the former; and from what has been before said, it appears, that this latter circumstance ought rather to have made the condensed liquor less acid; and yet it was found to be much more so, which shews strongly that it was the phlogisticated air which furnished the acid.

As a further confirmation of this point, these two comparative experiments were repeated with a little variation, namely, in the first experiment there was first let into the globe 1500 of dephlogisticated air, and then the mixture, consisting of 12200 of dephlogisticated air and 25900 of inflammable, was let in at different times as usual. In the second experiment, besides the 1500 of dephlogisticated air first let in, there was also admitted 2500 of phlogisticated air, after which the mixture, consisting of the same quantities of dephlogisticated and inflammable air as before, was let in as usual. The condensed liquor of the second experiment was about three times as acid as that of the first, as is required 119 grains of a diluted solution of salt of tartar to saturate it, and the other only 37. The standard of the burnt air was 0,78 in the second experiment, and 1,96 in the first.

The intention of previously letting in some dephlogisticated air in the two last experiments was, that the condensed liquor

was expected to become more acid thereby, as proved actually to be the case.

In the first of these two experiments, in order that the air to be exploded should be as free as possible from common air, the globe was first filled with a mixture of dephlogisticated and inflammable air, it was then exhausted, and the air to be exploded let in; by which means, though the globe was not perfectly exhausted, very little common air could be left in it. In the first set of experiments this circumstance was not attended to, and the purity of the dephlogisticated air was forgot to be examined in both sets.

From what has been said there seems the utmost reason to think, that dephlogisticated air is only water deprived of its phlogiston, and that inflammable air, as was before said, is either phlogisticated water, or else pure phlogiston; but in all probability the former.

As Mr. WATT, in a paper lately read before this Society, supposes water to consist of dephlogisticated air and phlogiston deprived of part of their latent heat, whereas I take no notice of the latter circumstance, it may be proper to mention in a few words the reason of this apparent difference between us. If there be any such thing as elementary heat, it must be allowed that what Mr. WATT says is true; but by the same rule we ought to say, that the diluted mineral acids consist of the concentrated acids united to water and deprived of part of their latent heat; that solutions of sal ammoniac, and most other neutral salts, consist of the salt united to water and elementary heat; and a similar language ought to be used in speaking of almost all chemical combinations, as there are very few which are not attended with some increase or diminution of heat. Now I have chosen to avoid this form of speaking,
both

both because I think it more likely that there is no such thing as elementary heat, and because saying so in this instance, without using similar expressions in speaking of other chemical unions, would be improper, and would lead to false ideas; and it may even admit of doubt, whether the doing it in general would not cause more trouble and perplexity than it is worth.

There is the utmost reason to think, that dephlogisticated and phlogisticated-air, as M. LAVOISIER and SCHEELE suppose, are quite distinct substances, and not differing only in their degree of phlogistication; and that common air is a mixture of the two; for if the dephlogisticated air is pretty pure, almost the whole of it loses its elasticity by phlogistication, and, as appears by the foregoing experiments, is turned into water, instead of being converted into phlogisticated air. In most of the foregoing experiments, at least $\frac{4}{7}$ ths of the whole was turned into water; and by treating some dephlogisticated air with liver of sulphur, I have reduced it to less than $\frac{1}{7}$ th of its original bulk, and other persons, I believe, have reduced it to a still less bulk; so that there seems the utmost reason to suppose, that the small residuum which remains after its phlogistication proceeds only from the impurities mixed with it.

It was just said, that some dephlogisticated air was reduced by liver of sulphur to $\frac{1}{7}$ th of its original bulk; the standard of this air was 4,8, and consequently the standard of perfectly pure dephlogisticated air should be very nearly 5, which is a confirmation of the foregoing opinion; for if the standard of pure dephlogisticated air is 5, common air must, according to this opinion, contain one-fifth of it, and therefore ought to lose one-fifth of its bulk by phlogistication, which is what it is actually found to lose.

From

From what has been said, it follows, that instead of saying air is phlogisticated or dephlogisticated by any means, it would be more strictly just to say, it is deprived of, or receives, an addition of dephlogisticated air; but as the other expression is convenient, and can scarcely be considered as improper, I shall still frequently make use of it in the remainder of this paper.

There seemed great reason to think, from Dr. PRIESTLEY's experiments, that both the nitrous and vitriolic acids were convertible into dephlogisticated air, as that air is procured in the greatest quantity from substances containing those acids, especially the former. The foregoing experiments, however, seem to shew that no part of the acid is converted into dephlogisticated air, and that their use in preparing it is owing only to the great power which they possess of depriving bodies of their phlogiston. A strong confirmation of this is, that red precipitate, which is one of the substances yielding dephlogisticated air in the greatest quantity, and which is prepared by means of the nitrous acid, contains in reality no acid. This I found by grinding 400 grains of it with spirits of sal ammoniac, and keeping them together for some days in a bottle, taking care to shake them frequently. The red colour of the precipitate was rendered pale, but not entirely destroyed; being then washed with water and filtered, the clear liquor yielded on evaporation not the least ammoniacal salt.

It is natural to think, that if any nitrous acid had been contained in the red precipitate, it would have united to the volatile alkali and have formed ammoniacal nitre, and would have been perceived on evaporation; but in order to determine more certainly whether this would be the case, I dried some of the same solution of quicksilver from which the red precipitate was prepared with a less heat, so that it acquired only an orange colour,

colour, and treated the same quantity of it with volatile alkali in the same manner as before. It immediately caused an effervescence, changed the colour to grey, and yielded 52 grains of ammoniacal nitre. There is the utmost reason to think, therefore, that red precipitate contains no nitrous acid; and consequently that, in procuring dephlogisticated air from it, no acid is converted into air; and it is reasonable to conclude, therefore, that no such change is produced in procuring it from any other substance.

It remains to consider in what manner these acids act in producing dephlogisticated air. The way in which the nitrous acid acts, in the production of it from red precipitate, seems to be as follows. On distilling the mixture of quicksilver and spirit of nitre, the acid comes over, loaded with phlogiston, in the form of nitrous vapour, and continues to do so till the remaining matter acquires its full red colour, by which time all the nitrous acid is driven over, but some of the watery part still remains behind, and adheres strongly to the quicksilver; so that the red precipitate may be considered, either as quicksilver deprived of part of its phlogiston, and united to a certain portion of water, or as quicksilver united to dephlogisticated air*; after which, on further increasing the heat, the water in it rises deprived of its phlogiston, that is, in the form of dephlogisticated

* Unless we were much better acquainted than we are with the manner in which different substances are united together in compound bodies, it would be ridiculous to say, that it is the quicksilver in the red precipitate which is deprived of its phlogiston, and not the water, or that it is the water and not the quicksilver; all that we can say is, that red precipitate consists of quicksilver and water, one or both of which are deprived of part of their phlogiston. In like manner, during the preparation of the red precipitate, it is certain that the acid absorbs phlogiston, either from the quicksilver or the water; but we are by no means authorized to say from which.

air,

air, and at the same time the quicksilver distills over in its metallic form. It is justly remarked by Dr. PRIESTLEY, that the solution of quicksilver does not begin to yield dephlogisticated air till it acquires its red colour.

Mercurius calcinatus appears to be only quicksilver which has absorbed dephlogisticated air from the atmosphere during its preparation; accordingly, by giving it a sufficient heat, the dephlogisticated air is driven off, and the quicksilver acquires its original form. It seems therefore that mercurius calcinatus and red precipitate, though prepared in a different manner, are very nearly the same thing.

From what has been said it follows, that red precipitate and mercurius calcinatus contain as much phlogiston as the quicksilver they are prepared from; but yet, as uniting dephlogisticated air to a metal comes to the same thing as depriving it of part of its phlogiston and adding water to it, the quicksilver may still be considered as deprived of its phlogiston; but the imperfect metals seem not only to absorb dephlogisticated air during their calcination, but also to be really deprived of part of their phlogiston, as they do not acquire their metallic form by driving off the dephlogisticated air.

In procuring dephlogisticated air from nitre, the acid acts in a different manner; as, upon heating the nitre red-hot, the dephlogisticated air rises mixed with a little nitrous acid, and at the same time the acid remaining in the nitre becomes very much phlogisticated; which shews that the acid absorbs phlogiston from the water in the nitre, and becomes phlogisticated, while the water is thereby turned into dephlogisticated air. On distilling 3155 grains of nitre in an unglazed earthen retort, it yielded 256000 grain measures of dephlogisticated air*, the
standard

* This is, about eighty-one grain measures from one grain of nitre; and the weight

standard of different parts of which varied from 3 to 3,65, but at a medium was 3,35. The matter remaining in the retort dissolved readily in water, and tasted alkaline and caustic. On adding diluted spirit of nitre to the solution, strong red fumes were produced; a sign that the acid in it was very much phlogisticated, as no fumes whatever would have been produced on adding the same acid to a solution of common nitre; that part of the solution also which was supersaturated with acid became blue; a colour which the diluted nitrous acid is known to assume when much phlogisticated. The solution, when saturated with this acid, lost its alkaline and caustic taste, but yet tasted very different from true nitre, seeming as if it had been mixed with sea-salt, and also required much less water to dissolve it; but on exposing it for some days to the air, and adding fresh acid as fast as by the flying off of the fumes the alkali predominated, it became true nitre, unmixed, as far as I could perceive, with any other salt*.

It has been remarked, that the dephlogisticated air procured from nitre is less pure, than that from red precipitate and many other substances, which may perhaps proceed from unglazed earthen retorts having been commonly used for this purpose, and which, conformably to Dr. PRIESTLEY's discovery, may possibly absorb some common air from without, and emit it along with the dephlogisticated air; but if it should be found that the dephlogisticated air procured from nitre in glass or glazed earthen vessels is also impure, it would seem to shew that part

weight of the dephlogisticated air, supposing it 800 times lighter than water, is one tenth of that of the nitre. In all probability it would have yielded a much greater quantity of air, if a greater heat had been applied.

* This phlogistication of the acid in nitre by heat has been observed by Mr. SCHRÖDER; see his experiments on air and fire, p. 45. English translation.

of the acid in the nitre is turned into phlogisticated air, by absorbing phlogiston from the watery part.

From what has been said it appears, that there is a considerable difference in the manner in which the acid acts in the production of dephlogisticated air from red precipitate and from nitre; in the former case the acid comes over first, leaving the remaining substance deprived of part of its phlogiston; in the latter the dephlogisticated air comes first, leaving the acid loaded with the phlogiston of the water from which it was formed.

On distilling a mixture of quicksilver and oil of vitriol to dryness, part of the acid comes over, loaded with phlogiston, in the form of volatile sulphureous acid and vitriolic acid air; so that the remaining white mass may be considered as consisting of quicksilver deprived of its phlogiston, and united to a certain proportion of acid and water, or of plain quicksilver united to a certain proportion of acid and dephlogisticated air. Accordingly on urging this white mass with a more violent heat, the dephlogisticated air comes over, and at the same time part of the quicksilver rises in its metallic form, and also part of the white mass, united in all probability to a greater proportion of acid than before, sublimes; so that the rationale of the production of dephlogisticated air from turbith mineral, and from red precipitate, are nearly similar.

True turbith mineral consists of the abovementioned white mass, well washed with water, by which means it acquires a yellow colour, and contains much less acid than the unwashed mass. Accordingly it seems likely, that on exposing this to heat, less of it should sublime without being decomposed, and consequently that more dephlogisticated air should be procured from it than from the unwashed mass.

This

This is an instance, that the superabundant vitriolic acid may, in some cases, be better extracted from the base it is united to by water than by heat. Vitriolated tartar is another instance; for, if vitriolated tartar be mixed with oil of vitriol and exposed even to a pretty strong red heat, the mass will be very acid; but, if this mass is dissolved in water, and evaporated, the crystals will be not sensibly so.

In all probability, the vitriolic acid acts in the same manner in the production of dephlogisticated air from alum, as the nitrous does in its production from nitre; that is, the watery part comes over first in the form of dephlogisticated air, leaving the acid charged with its phlogiston. Whether this is also the case with regard to green and blue vitriol, or whether in them the acid does not rather act in the same manner as in turbith mineral, I cannot pretend to say, but I think the latter more likely.

There is another way by which dephlogisticated air has been found to be produced in great quantities, namely, the growth of vegetables exposed to the sun or day-light; the rationale of which, in all probability, is, that plants, when assisted by the light, deprive part of the water sucked up by their roots of its phlogiston, and turn it into dephlogisticated air, while the phlogiston unites to, and forms part of, the substance of the plant.

There are many circumstances which shew, that light has a remarkable power in enabling one body to absorb phlogiston from another. Mr. SENEBIER has observed, that the green tincture procured from the leaves of vegetables by spirit of wine, quickly loses its colour when exposed to the sun in a bottle not more than one third part full, but does not do so in the dark, or if the bottle is quite full of the tincture, or if the air in it

is phlogisticated; whence it is natural to conclude, that the light enables the dephlogisticated part of the air to absorb phlogiston from the tincture; and this appears to be really the case, as I find that the air in the bottle is considerably phlogisticated thereby. Dephlogisticated spirit of nitre also acquires a yellow colour, and becomes phlogisticated, by exposure to the sun's rays*; and I find on trial that the air in the bottle in which it is contained becomes dephlogisticated, or, in other words, receives an increase of dephlogisticated air, which shews that the change in the acid is not owing to the sun's rays communicating phlogiston to it, but to their enabling it to absorb phlogiston from the water contained in it, and thereby to produce dephlogisticated air. Mr. SCHEELE also found, that the dark colour acquired by luna cornea on exposure to the light, is owing to part of the silver being revived; and that gold, dissolved in aqua regia and deprived by distillation of the nitrous and superfluous marine acid, is revived by the same means; and there is the utmost reason to think, that, in both cases, the revival of the metal is owing to its absorbing phlogiston from the water.

Vegetables seem to consist almost intirely of fixed and phlogisticated air, united to a large proportion of phlogiston and some water, since by burning in the open air, in which their phlogiston unites to the dephlogisticated part of the atmosphere and forms

* If spirit of nitre is distilled with a very gentle heat, the part which comes over is high coloured and fuming, and that which remains behind is quite colourless, and fumes much less than other nitrous acid of the same strength, and the fumes are colourless. This is called dephlogisticated spirit of nitre, as it appears to be really deprived of phlogiston by the process. The manner of preparing it, as well as its property of regaining its yellow colour by exposure to the light, is mentioned by Mr. SCHEELE in the Stockholm Memoirs, 1774.

water,

water, they seem to be reduced almost intirely to water and those two kinds of air. Now plants growing in water without earth, can receive nourishment only from the water and air, and must therefore in all probability absorb their phlogiston from the water. It is known also that plants growing in the dark do not thrive well, and grow in a very different manner from what they do when exposed to the light.

From what has been said, it seems likely that the use of light, in promoting the growth of plants and the production of dephlogisticated air from them, is, that it enables them to absorb phlogiston from the water. To this it may perhaps be objected, that though plants do not thrive well in the dark, yet they do grow, and should therefore, according to this hypothesis, absorb water from the atmosphere, and yield dephlogisticated air, which they have not been found to do. But we have no proof that they grew at all in any of those cases in which they were found not to yield dephlogisticated air; for though they will grow in the dark, yet their vegetative powers may perhaps at first be intirely checked by it, especially considering the unnatural situation in which they must be placed in such experiments. Perhaps too plants growing in the dark may be able to absorb phlogiston from water not much impregnated with dephlogisticated air, but not from water strongly impregnated with it; and consequently, when kept under water in the dark, may perhaps at first yield some dephlogisticated air, which, instead of rising to the surface, may be absorbed by the water, and, before the water is so much impregnated as to suffer any to escape, the plant may cease to vegetate, unless the water is changed. Unless therefore it could be shewn that plants growing in the dark, in water alone, will increase in size, without yielding dephlogisticated

gified air, and without the water becoming more impregnated with it than before, no objection can be drawn from thence.

Mr. SENEBIER finds, that plants yield much more dephlogistified air in distilled water impregnated with fixed air, than in plain distilled water, which is perfectly conformable to the abovementioned hypothesis; for as fixed air is a principal constituent part of vegetable substances, it is reasonable to suppose that the work of vegetation will go on better in water containing this substance, than in other water.

There are several memoirs of Mr. LAVOISIER published by the Academy of Sciences, in which he intirely discards phlogiston, and explains those phænomena which have been usually attributed to the loss or attraction of that substance, by the absorption or expulsion of dephlogistified air; and as not only the foregoing experiments, but most other phænomena of nature, seem explicable as well, or nearly as well, upon this as upon the commonly believed principle of phlogiston, it may be proper briefly to mention in what manner I would explain them on this principle, and why I have adhered to the other. In doing this, I shall not conform strictly to his theory, but shall make such additions and alterations as seem to suit it best to the phænomena; the more so, as the foregoing experiments may, perhaps, induce the author himself to think some such additions proper.

According to this hypothesis, we must suppose, that water consists of inflammable air united to dephlogistified air; that nitrous air, vitriolic acid air, and the phosphoric acid, are also combinations of phlogistified air, sulphur, and phosphorus, with dephlogistified air; and that the two former, by a further addition of the same substance, are reduced to the common
nitrous

nitrous and vitriolic acids; that the metallic calces consist of the metals themselves united to the same substance, commonly, however, with a mixture of fixed air; that on exposing the calces of the perfect metals to a sufficient heat, all the dephlogisticated air is driven off, and the calces are restored to their metallic form; but as the calces of the imperfect metals are vitrified by heat, instead of recovering the metallic form, it should seem as if all the dephlogisticated air could not be driven off from them by heat alone. In like manner, according to this hypothesis, the rationale of the production of dephlogisticated air from red precipitate is, that during the solution of the quicksilver in the acid and the subsequent calcination, the acid is decomposed, and quits part of its dephlogisticated air to the quicksilver, whereby it comes over in the form of nitrous air, and leaves the quicksilver behind united to dephlogisticated air, which, by a further increase of heat, is driven off, while the quicksilver re-assumes its metallic form. In procuring dephlogisticated air from nitre, the acid is also decomposed; but with this difference, that it suffers some of its dephlogisticated air to escape, while it remains united to the alkali itself, in the form of phlogisticated nitrous acid. As to the production of dephlogisticated air from plants, it may be said, that vegetable substances consist chiefly of various combinations of three different bases, one of which, when united to dephlogisticated air, forms water, another fixed air, and the third phlogisticated air; and that by means of vegetation each of these substances are decomposed, and yield their dephlogisticated air; and that in burning they again acquire dephlogisticated air, and are restored to their pristine form.

It seems, therefore, from what has been said, as if the phenomena of nature might be explained very well on this principle,

ciple, without the help of phlogiston; and indeed, as adding dephlogisticated air to a body comes to the same thing as depriving it of its phlogiston and adding water to it, and as there are, perhaps, no bodies entirely destitute of water, and as I know no way by which phlogiston can be transferred from one body to another, without leaving it uncertain whether water is not at the same time transferred, it will be very difficult to determine by experiment which of these opinions is the truest; but as the commonly received principle of phlogiston explains all phænomena, at least as well as Mr. LAVOISIER's, I have adhered to that. There is one circumstance also, which though it may appear to many not to have much force, I own has some weight with me; it is, that as plants seem to draw their nourishment almost intirely from water and fixed and phlogisticated air, and are restored back to those substances by burning, it seems reasonable to conclude, that notwithstanding their infinite variety they consist almost intirely of various combinations of water and fixed and phlogisticated air, united according to one of these opinions to phlogiston, and deprived according to the other of dephlogisticated air; so that, according to the latter opinion, the substance of a plant is less compounded than a mixture of those bodies into which it is resolved by burning; and it is more reasonable to look for great variety in the more compound than in the more simple substance.

Another thing which Mr. LAVOISIER endeavours to prove is, that dephlogisticated air is the acidifying principle. From what has been explained it appears, that this is no more than saying, that acids lose their acidity by uniting to phlogiston, which with regard to the nitrous, vitriolic, phosphoric, and arsenical acids, is certainly true. The same thing, I believe, may be said of the acid of sugar; and Mr. LAVOISIER's experiment is a
strong

Strong confirmation of BERGMAN's opinion, that none of the spirit of nitre enters into the composition of the acid, but that it only serves to deprive the sugar of part of its phlogiston. But as to the marine acid and acid of tartar, it does not appear that they are capable of losing their acidity by any union with phlogiston. It is to be remarked also, that the acids of sugar and tartar, and in all probability almost all the vegetable and animal acids, are by burning reduced to fixed and phlogisticated air, and water, and therefore contain more phlogiston, or less dephlogisticated air, than those three substances.

XIV. *Remarks on Mr. Cavendish's Experiments on Air. In a Letter from Richard Kirwan, Esq. F. R. S. to Sir Joseph Banks, Bart. P. R. S.*

Read Feb. 5, 1784.

S I R,

HAVING listened with much attention, and derived much useful information from the very curious experiments of Mr. CAVENDISH, read at our last meeting, it is with peculiar regret I feel myself withheld from yielding an intire assent to all he has advanced in his very ingenious paper; and it is with still greater that I find myself obliged, by reason of the opposition of some of his deductions to those I had the honour to lay before the society about two years ago, to expose the reasons of my dissent, through your mediation, before this meeting.

In the paper already mentioned, read in April, 1782, I attributed the diminution of respirable air, observed in common phlogistic processes, to the generation and absorption of fixed air, which is now known to be an acid, and capable of being absorbed by several substances. That fixed air was some how or other produced in phlogistic processes, either by *separation* or *composition*, I took for granted from the numerous experiments of Dr. PRIESTLEY; and among these I selected, as least liable to objection, the Calcination of Metals, the decomposition

tion of nitrous by mixture with respirable air, the phlogistication of respirable air by the electric spark, and, lastly, that effected by amalgamation. In each of these instances Mr. CAVENDISH is of opinion, that the diminution of respirable air is owing to the production of water, which, according to him, is formed by the union of the phlogiston, disengaged in those processes, with the dephlogisticated part of common air; and that fixed air is never produced in phlogistic processes, except some animal or vegetable substance is concerned in the operation, from whose decomposition it may arise. To which of these causes the diminution of respirable air is to be attributed, I shall now endeavour to elucidate.

Of the Calcination of Metals.

I attributed the diminution of air by the calcination of metals, to the conversion of the dephlogisticated part of common air into fixed air, by reason of its union with the phlogiston of the metal, for this plain reason, because I find it acknowledged on all hands, that the calces of all the base metals yield fixed air, when sufficiently heated. Mr. Cavendish allows the fact in general, but ascribes the fixed air found in them to their long exposure to the atmosphere, in which he says fixed air pre-exists; but that it exists in common air in any quantity worth attending to, or is extracted from it in any degree, I take the liberty of denying, grounded on the following facts. First, I have frequently agitated 18 cubic inches of common air in 2 of lime-water, and 2 of common air in 18 of lime-water, but could never perceive the slightest milkiness; and yet the thousandth part of a cubic inch of fixed air would thus be made sensible; for if a cubic inch of it be dissolved in 3 ounces of

water, a few drops of that water let into lime-water will produce a cloud. Mr. FONTANA says, he frequently agitated 1 cubic inch of Tincture of Turnsole in 7 or 800 of common air, without reddening it (23 Roz. p. 188.); and yet, according to Mr. BERGMAN, 1 cubic inch of fixed air is sufficient to redden 50 of Tincture of Turnsole (1 BERGM. 11.); from whence I am apt to think, that 700 cubic inches of common air do not even contain $\frac{1}{700}$ th of a cubic inch of fixed air. Dr. WHYTT found that 12 ounces of strong lime-water, being exposed to the open air for 19 days, still retained about 1 grain of lime, (on Lime-water, p. 32.). Now 12 ounces of strong lime-water contain at most 9,5 grains of lime, and 1 grain of lime requires only 0,56 of a cubic inch of fixed air to precipitate it, the thermometer at 55 and the barometer at 29,5, as I have found. Therefore in 19 days this lime-water did not come in contact with more than four cubic inches of fixed air; yet it is certain that a large quantity of fixed air is continually disengaged, and thrown into the atmosphere, by various processes, as putrefaction, combustion, &c. but it seems equally certain that it is either decomposed, or more probably absorbed by various bodies. Mr. FONTANA let loose 20000 cubic inches of fixed air, in a room whose windows and doors were closed, yet in half an hour after he could not discover the least trace of it (ibid.). Though fixed air perpetually oozes from the floor of the *Grotto del Cane*, yet at the distance of four or five feet from the ground none is found; animals may live, lights burn, &c. (Roz. Ibid. Mem. Stockh. 1775.). If distilled water be exposed to the atmosphere, it is never found to absorb fixed air, but rather dephlogisticated air, according to Mr. SCHEELÉ's experiments, which could never happen if the atmosphere contained any sensible proportion of fixed

fixed air; nor has rain-water been ever found to contain any, which it certainly should on the same hypothesis; even Mr. CAVENDISH himself could find no fixed air in the residuum or products of about 1040 ounce measures of common air, which he burnt with inflammable air.

It is true, Dr. PRIESTLEY supposed common air to contain $\frac{1}{11}$ of its bulk of fixed air; but he drew this conclusion not from any direct experiment, but from the quantity of fixed air produced by breathing, which he at that time believed to have been barely precipitated, and not generated, an opinion which he has found reason to alter from his own experiments. I think I may therefore conclude, that the quantity of fixed air contained in the atmosphere is absolutely inappreciable.

Secondly, supposing the atmosphere to contain a very small proportion of fixed air, yet I do not think it can be inferred that metals, during their calcination, extract any, because I find that lime exposed to red heat ever so long extracts none, though it is formed by a calcination in open air, which lasts at least as long as that of any metal; neither does precipitate *per se* attract any, though its calcination lasts several months; nor does this proceed from the want of affinity, for if a saturated solution of mercury in any of the acids be precipitated by a mild vegetable alkali, very little effervescence is perceived, and the precipitate weighs much more than the quantity of mercury employed, and that this increase of weight arises in part from the fixed air absorbed will presently be seen.

Since then metals may be calcined in close vessels, since they then absorb one fourth part of the common air to which they are exposed, since all metallic calces (except those of mercury, which I shall presently mention) yield fixed air, since common

air

air contains scarce any fixed air; is it not apparent that the fixed air thus found was generated by the very act of calcination, by the union of the phlogiston of the metal with the dephlogisticated part of the common air, since after the operation the metal is deprived of its phlogiston, and the air of its dephlogisticated part?

But Mr. CAVENDISH objects, that no one has extracted fixed air from metals calcined in close vessels. To which I answer, that this further proof is difficult, and no way necessary; it is difficult, because the operation can easily be performed only on small quantities; it is unnecessary, because it differs from the operation in open air only by the quantities of the materials employed, in every other respect it is exactly the same. Since Mr. CAVENDISH suspects the results are different, it is incumbent on him to shew that difference; but until then, according to Sir ISAAC NEWTON's second rule, *to natural effects of the same kind the same causes are to be assigned, as far as it may be done*, that is, until experience points out some other cause.

It may further be urged, that precipitate *per se* yields only dephlogisticated air, that minium also yields a large proportion of it. This difficulty I have formerly answered by asserting, that these calces are in fact united only to fixed air, and that they yield dephlogisticated air, merely because the fixed air is decomposed by the total or partial revivification of the metallic substances; this I think may be demonstrated by the following experiments. Let sublimate corrosive singly be treated in any manner, it will not yield dephlogisticated air (4 Pr. 240.); but let a solution of sublimate corrosive be precipitated by a mild fixed alkali, this precipitate washed, dried, and distilled in a pneumatic apparatus, will yield dephlogisticated air, and the

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mercury will be revived; but, if the solution of sublimate corrosive be precipitated by lime-water, it seems no air will be produced. Here then we see, 1st, that the calx of mercury unites with fixed air; and, 2dly, that this fixed air is, during the revivification of the mercury, converted into dephlogisticated air. Again: let one ounce of red precipitate, which, according to Mr. CAVENDISH, contains no nitrous acid, be distilled with two ounces of filings of iron; this quantity of precipitate, which, if distilled by itself, would yield 60 ounce measures of dephlogisticated air, will, when distilled with this proportion of filings of iron, yield 40 ounce measures of fixed air, as Dr. PRIESTLEY has shewn in his last paper: whichever way this is explained, some or other of my opinions are confirmed; for either the mercurial calx is already combined with fixed air (which I believe to be the case), and this air passes undecomposed, because the mercury extracts phlogiston from the iron; or it contains dephlogisticated air, which is converted into fixed air by its union with the phlogiston of the iron.

If precipitate *per se* be digested in marine acid, the mercury will be revived (3. BERGM. 415.). Now this calx does not dephlogisticate the marine acid; for this acid, when dephlogisticated, dissolves mercury; how then does it revive it, if not by expelling the fixed air contained in it, which in the moment of its expulsion is decomposed, leaving its phlogiston to the mercury, which is thereby revived?

Again: if litharge be heated in a gun-barrel, it will afford more fixed and less dephlogisticated air than if heated in glass or earthen vessels. Does not this happen, because the calx of lead, receiving some phlogiston from the metal, does not dephlogisticate so great a proportion of the fixed air as it otherwise would?

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Further: there is no substance which yields dephlogisticated air, but yields also fixed air, even precipitate *per se* not excepted; (3 PRIEST. 16.) and what is remarkable, they all yield fixed air first, and dephlogisticated air only towards the end of the process. Does not this happen because metallic calces attract phlogiston so much more strongly, as they are more heated? Thus many calciform iron ores become magnetic by calcination, though they were not so before; so also do all the calces of iron when exposed to the focus of a burning glass (5 Dict. Chy. 179). Thus mercury cannot be calcined but in a heat inferior to that in which it boils; thus minium cannot be formed but in a moderate heat, and if heated still more it returns to the state of massicot, in which it was before it became minium, and much of it is reduced. So if a solution of luna cornea in volatile alkali be triturated with mercury, the silver will be revived, and the marine acid unite to the mercury, which shews this acid has a stronger attraction to Mercury than to silver; yet if sublimate corrosive and silver be distilled in a strong heat, the mercury will be revived, and the marine acid unite to the silver, which shews that the attraction of mercury to phlogiston increases with the heat applied.

Before I conclude this head, I will mention another experiment, which I think decisive in favour of my opinion of the composition of fixed air. If filings of zinc be digested in a caustic fixed alkali in a gentle heat, the zinc will be dissolved with effervescence, and the alkali will be rendered in great measure mild. But if, instead of filings of zinc, flowers of zinc be used, and treated in the same manner, there will be no solution, and the alkali will remain caustic. In the first case the effervescence arises from the production of inflammable air, which

which phlogisticates the common air contiguous to it, and produces fixed air, which is immediately absorbed by the alkali, and renders it mild. In the second case, no inflammable air is produced, the common air is not phlogisticated, and consequently the alkali remains caustic*. This experiment also proves that metallic calces attract fixed air more strongly than alkalis attract it; for the calces of zinc are known to contain fixed air, and yet alkalis digested with them remain caustic; and this accounts for the slight turbidity of lime-water when metals are calcined over it; for as soon as the phlogiston is disengaged from the metal, and before it has absorbed the whole quantity of fire requisite to throw it into the form of inflammable air, it meets with the dephlogisticated part of the common air on the surface of the metal, and there forms fixed air, which is instantly absorbed by the calx with which it is in contact, so that it is not to be wondered that it does not unite to the lime from which it is distant.

Of the Decomposition of Nitrous Air by mixture with Common Air.

AS soon as I had heard Mr. CAVENDISH's paper read, I set about trying whether lime would be precipitated from lime-water during the process, an experiment I had never made before with common air, taking it for granted that it was so, from the repeated experiments of Dr. PRIESTLEY, and indeed of all others who had treated this subject†: and, in effect,

* See Mr. LASSIGNY's Experiments on air, Mem. Par. 1797. p. 7 & 8.

† See 2 Pr. 114. 189. 2 Pr. 218. Font. Recherches Phys. p. 77. 1 Chy. Di. 314.

when I made the experiment with nitrous air prepared and confined by the water of my tub, I found lime-water admitted to it instantly precipitated. But after I had read Mr. CAVENDISH's paper, which he had the politeness to permit me, and had, according to his direction, received the nitrous air over lime-water, I did not then perceive the least milkiness after admitting common air. After 12 hours I indeed perceived a whitish dust, on the bottom of the glass vessel in which I made the experiment, which I cannot assure to be calcareous; and, on breathing into the lime-water, an evident milkiness ensued; so that I little doubt but the precipitation I observed in the first experiment arose from the decomposition of the aerial selenite contained in the water of the tub. And it is very possible that the precipitation of lime, which I perceived some years ago on mixing dephlogisticated air and nitrous air, might have arisen from the same cause, or from fixed air pre-contained in the dephlogisticated, as this last had not been washed in lime-water. Yet I do not think the failure of this experiment at all conclusive against the supposed production of fixed air on this occasion, because the quantity of fixed air is so small, that it may well be supposed to unite to the nitrous selenite formed in the lime-water. It is well known that a small quantity of fixed air is capable of uniting to all neutral salts: thus Dr. PRIESTLEY has extracted it from tartar vitriolate and alum, (2 PR. 115, 116.) and gypsum, (2 PR. 80.); and Dr. MAC BRIDE found it in nitre and common salt, though in small quantity. But to try whether nitrous selenite would attract any, I made a solution of chalk in nitrous acid, which, when saturate, weighed 381,25 grains; but, being exposed to the air for a few hours, it weighed 382,25. I afterwards took a very dilute nitrous acid, in which an acid taste was barely perceptible, and impregnated it with a very small proportion

portion of fixed air, and then let fall a few drops of it into lime-water; not the smallest cloud was perceived, and yet when I breathed into it afterwards it became milky in a few seconds; so that this experiment is perfectly analogous to that in which nitrous and common air were mixed.

But if nitrous air and common air be mixed over dry mercury, the result is intirely adverse to the opinion of Mr. CAVENDISH, and favourable to mine; for in this case the common air is not at all diminished until water is admitted to it, and the mixture agitated a few minutes, and then the diminution is nearly the same as if the mixture were made over water. Thus when I mixed two cubic inches of common air with one of nitrous air, they occupied the space of two inches and one-eighth; and the surface of the mercury was immediately calcined; which shews that the inch of nitrous air was decomposed, and produced nitrous acid; but the common air was undiminished; and the one eighth of an inch over and above the two inches of common air, proceeded from an addition of new nitrous air, formed by the corrosion of the surface of the mercury. That the common air should remain undiminished is easily explained in my system, because fixed air is formed, which, on this occasion, must remain unabforbed, at least for a long time, as there is nothing at hand that can immediately receive it; and hence, if water be admitted soon after the mixture of both airs, the diminution will be nearly the same as if the mixture had been originally made over water, though not exactly the same; because the nitrous air, produced by the union of the newly formed nitrous acid with the mercury, is not entirely abforbable by water. But, in Mr. CAVENDISH'S hypothesis, the common air should be diminished just as much as if the mixture were made over water; for, according to him,

this diminution arises from the conversion of the dephlogistified part of the common air into water, which water should immediately unite to the nitrous salt of mercury, and leave the common air lessened in its bulk by a portion commensurate to that converted into water, or, if he will not allow the water to have immediately united to the mercurial salt, at least by the difference of the bulk of the water produced, and that of an equal weight of the common air converted into it: but neither happens; for the common air is not at all diminished; nor can he explain, consistently with his system, why the admission of water should immediately produce a diminution in the common air, as, according to him, it contains nothing that can be absorbed. Dr. PRIESTLEY has remarked, that if a mixture of both airs be suffered to stand several hours, even the admission of water will produce no diminution. This is owing to two causes; 1st, because a large quantity of nitrous air is produced, by the continued action of the concentrated nitrous acid newly formed; and, 2dly, because the fixed air, on whose absorption the diminution depends, is absorbed by the mercurial salt, as may be inferred from the experiment in LAVOISIER, p. 248.

Of the Diminution of Common Air by the Electric Spark.

Of all the instances of the artificial production of fixed air, by the union of phlogiston with the dephlogistified part of common air, there is none perhaps so convincing, as that exhibited by taking the electric spark through common air, over a solution of litmus, or lime-water; for the common air is diminished one fourth, the litmus reddened, and the lime-water precipitated. Mr. CAVENDISH indeed attributes the redness of the

the litmus to fixed air; but he thinks it proceeds from a decomposition of some part of the vegetable juice, as all vegetable juices contain fixed air. Yet that such a decomposition does not take place, I think may be inferred from the following reasons: first, if the electric spark be taken through phlogisticated or inflammable air confined by litmus, no redness is produced, the air not being in the least diminished; and, adly, if the litmus were decomposed, inflammable air should be produced as well as fixed air; and then there should be an addition of bulk instead of a diminution; but what sets the origin of the fixed air from the phlogistication of the common air beyond all doubt is, that if lime-water be used instead of litmus, the diminution is the same, and the lime is precipitated. Here Mr. CAVENDISH says, the fixed air proceeds either from *some dirt in the tube*; a supposition, which, being neither necessary nor probable, is not admissible; or else *from some combustible matter in the lime*; but lime contains no combustible matter, except perhaps phlogiston, which cannot produce fixed air but by uniting to the common air, according to my supposition; but it is much more probable, that the diminution does not arise from any phlogiston in the lime, as it is exactly the same whether lime-water be used or not; and the lime does not appear to be in the least altered, and in fact contains scarce any phlogiston.

Of the diminution of Common Air, by the Amalgamation of Mercury and Lead.

I attributed this diminution to the phlogistication of the common air by the process of amalgamation, and the consequent production and absorption of fixed air. On this Mr. CA-

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VENDISH observes, "that mercury, fouled by the addition of lead or tin, deposits a powder which consists in great measure of the calx of the metal: he found also, that some powder of this sort contained fixed air; but it is not clear that this air was produced by the phlogistication of the air in which the mercury was shaken, as the powder was not prepared on purpose, but was formed from mercury fouled by having been used for various purposes, and may therefore contain other impurities, besides the metallic calx." On this I remark, that Dr. PRIESTLEY did not indeed at first prepare this powder on purpose; but he afterwards did so prepare it (4 PRIEST. p. 148, 149.) and obtained a powder exactly of the same sort; and it is certain that the fixed air found in it proceeded from the common air, both because metallic calces, not formed by amalgamation, will not unite with mercury, as is well known; and because this calx cannot be formed by agitation of the mercury and lead, in phlogisticated, inflammable, or any other air which is not respirable; and the fixed air cannot proceed from any impurity, as mercury will not unite in its running form to any other but metallic substances, which it always partially dephlogisticates, like other menstruums (3 Chy. Dijon, 425.).

Of the Diminution of Respirable Air by Combustion.

Though I have no doubt but the diminution of respirable air, by the combustion of sulphur and phosphorus, proceeds also in great measure from the production and absorption of fixed air, yet I avoided mentioning this operation, as the presence of a stronger acid renders the presence of a weaker impossible to be proved, more especially, as both these acids precipitate lime from lime-water; but the great increase of weight which the phos-

phosphoric acid gains is a strong additional inducement to think that it absorbs fixed air. During the combustion of vegetable substances, I think it highly probable that fixed air is formed, both from my own experiments on the combustion of wax candles, and that mentioned in the first volume of Dr. PRIESTLEY's Observations, p. 136; but when inflammable air from metals and dephlogisticated air are fired, as a great diminution takes place, and yet no fixed air is found, I am nearly convinced, by Mr. CAVENDISH's experiments, that water is really produced; nor am I surprized that, in this instance, the union of phlogiston and dephlogisticated air should form a compound very different from that which it forms in other instances of phlogistication, but should rather be led to expect it *a priori*; for in this case the phlogiston is in its most rarefied known state, and unites to dephlogisticated air, the substance to which it has the greatest affinity, in circumstances the most favourable to the closest and most intimate union; for both, in the act of inflammation, are rarefied to the highest degree; both give out their specific fire, the great obstacle to their union, it being by the inflammation converted into *sensible* heat (a circumstance which, in my opinion, constitutes the very essence of flame); the resulting compound having then lost the greatest part of its specific fire, is necessarily reduced, according to Dr. BLACK's theory, into a denser state, which the present experiment shews to be water; whereas, in common cases of combustion, the phlogiston being denser and less divided, unites less intimately with the dephlogisticated part of common air, consequently expels less of its specific fire, and therefore forms less dense compounds, *viz.* fixed and phlogisticated airs; and so much the more, as a great part intirely escapes combustion; but it seems probable

probable that in very strong and bright inflammations, the union is more perfect, and water formed.

Water being then the result of the closest and most intimate union of dephlogisticated air and phlogiston, it seems to me very improbable, that it is ever decomposed by the affinity of any acid to phlogiston, as all the experiments hitherto made seem to prove, that phlogiston has a stronger affinity to dephlogisticated air than to any other substance, except hot metallic calces; and these, in my opinion, are incapable of forming any union with water, except as far as they are saline, but they never can be reduced by it. So also water is incapable of uniting with any more phlogiston, as sulphur is, both being already saturated.

Mr. CAVENDISH is inclined to think, that pure inflammable air is not pure phlogiston, because it does not immediately unite with dephlogisticated air, when both airs are simply mixed with each other; this reason seems to me of no moment, because I see several other substances, that have the strongest affinity to each other, refuse to unite suddenly, or even at all, through the very same cause that dephlogisticated and inflammable airs refuse to unite; viz. on account of the specific fire which they contain, and must lose, before such union can take place: thus fixed air will never unite to dry lime, though they be kept ever so long together; thus, if water be poured on the strongest oil of vitriol, they will remain several weeks in contact, without uniting, as I myself have experienced; and yet, in both cases, the specific fire need be expelled only from one of the substances, and not from both: but after a long time they will unite; so also will inflammable and dephlogisticated air, as Dr. PRIESTLEY has discovered since his last publication.

That phlogisticated air should consist of supersaturated nitrous air, I think improbable, as it retains its phlogiston much more strongly than nitrous air, which, according to the general laws of affinities, it should not, if it contained an excess of phlogiston; and as Dr. PRIESTLEY and Mr. FONTANA repeatedly assure us, they have converted it into common air, by washing it in water, in contact with the atmosphere. I am, &c.

London,
Jan. 29, 1784.

R. KIRWAN.

XV. Answer to Mr. Kirwan's Remarks upon the Experiments on Air. By Henry Cavendish, Esq. F. R. S. and S. A.

Read March 4, 1784.

IN a paper lately read before this Society, containing many experiments on air, I gave my reasons for supposing that the diminution which respirable air suffers by phlogistication, is not owing either to the generation or separation of fixed air from it; but without any arguments of a personal nature, or which related to any one person who espouses the contrary doctrine more than to another. This being contrary to the opinion maintained by Mr. KIRWAN, he has written a paper in answer to it, which was read on the fifth of February. As I do not like troubling the Society with controversy, I shall take no notice of the arguments used by him, but shall leave them for the reader to form his own judgement of; much less will I endeavour to point out any inconsistencies or false reasonings, should any such have crept into it; but as there are two or three experiments mentioned there, which may perhaps be considered as disagreeing with my opinion, I beg leave to say a few words concerning them.

Mr. DE LASSONE found that filings of zinc, digested in a caustic fixed alkali, were partially dissolved with a small effervescence, and that the alkali was rendered in some measure

sure mild. This mildness of the alkali Mr. KIRWAN accounts for by supposing, that the inflammable air, which is separated during the solution, and causes the effervescence, unites to the atmospheric air contiguous to it, and thereby generates fixed air, which is absorbed by the alkali. But, in reality, the only circumstance from which Mr. DE LASSONE judged the alkali to become mild, was its making some effervescence when saturated with acids; and this effervescence is more likely to have proceeded from the expulsion of inflammable air than of fixed air, as it seems likely, that the zinc might be more completely deprived of its phlogiston by the acid than by the alkali.

In the abovementioned paper I say, Dr. PRIESTLEY observed, that quicksilver fouled by the addition of lead or tin, deposits a powder by agitation and exposure to the air, which consists in great measure of the calx of the imperfect metal. He found too some powder of this kind to contain fixed air; but it must be observed, that the powder used in this experiment was not prepared on purpose, but was procured from quicksilver fouled by having been used in various experiments, and may therefore have contained other impurities besides the metallic calces. On this Mr. KIRWAN remarks, that Dr. PRIESTLEY did not at first prepare this powder on purpose, but he afterwards did so prepare it (4 PR. p. 148. and 149.), and obtained a powder exactly of the same sort. It was natural to suppose from this remark, that Dr. PRIESTLEY must have obtained fixed air from the powder prepared on purpose, and that I had overlooked the passage; but, on turning to the pages referred to, I was surprised to find that it was otherwise, and that Dr. PRIESTLEY not so much as hints that he procured fixed air from the powder thus prepared.

With regard to the calcination of metals it may be proper to remark, that this operation is usually performed over the fire, by methods in which they are exposed to the fumes of the burning fuel, and which are so replete with fixed air, that it is not extraordinary, that the metallic calx should, in a short time, absorb a considerable quantity of it; and in particular red lead, which is the calx on which most experiments have been made, is always so prepared. There is another kind of calcination, however, called rusting, which is performed in the open air; but this is so slow an operation, that the rust may easily imbibe a sufficient quantity of fixed air, notwithstanding the small quantity of it usually contained in the atmosphere.

Mr. KIRWAN allows that lime-water is not rendered cloudy by the mixture of nitrous and common air; but contends that this does not prove that fixed air is not generated by the union, as he thinks it may be absorbed by the nitrous selenite produced by the union of the nitrous acid with the lime. This induced me to try how small a quantity of fixed air would be perceived in this experiment. I accordingly repeated it in the same manner as described in my paper, except that I purposely added a little fixed air to the common air, and found that when this addition was $\frac{1}{7}$ th of the bulk, or $\frac{1}{10}$ th of the weight of the common air, the effect on the lime-water was such as could not possibly have been overlooked in my experiments. But as those who suppose fixed air to be generated by the mixture of nitrous and common air, may object to this manner of trying the experiment, and say, that the quantity of fixed air absorbed by the lime-water was really more than $\frac{1}{7}$ th of the bulk of the common air, being equal to that quantity over and

and above the air generated by the mixture, I made another experiment in a different manner; namely, I filled a bottle with lime-water, previously mixed with as much nitrous acid as is contained in an equal bulk of nitrous air, and having inverted it into a vessel of the same, let up into it, in the same manner as in the above-mentioned experiments, a mixture of common air with $\frac{1}{3}$ th of its bulk of fixed air, until it was half full. The event was the same as before; namely, the cloudiness produced in the lime-water was such that I could not possibly have overlooked. It must be observed, that in this experiment no fixed air could be generated, and a still greater proportion of the lime-water was turned into nitrous selenite than in the above-mentioned experiments; so that we may safely conclude, that if any fixed air is generated by the mixture of common and nitrous air, it must be less than $\frac{1}{3}$ th of the bulk of the common air.

As for the nitrous selenite, it seems not to make the effect of the fixed air at all less sensible, as I found by filling two bottles with common air mixed with $\frac{1}{10}$ th of its bulk of fixed air, and pouring into each of them equal quantities of diluted lime-water; one of these portions of lime-water being previously diluted with an equal quantity of distilled water, and the other with the same quantity of a diluted solution of nitrous selenite, containing about $\frac{1}{10}$ th of its weight of calcareous earth; when I could not perceive that the latter portion of lime-water was rendered at all less cloudy than the former. Though the nitrous selenite, however, does not make the effect of the fixed air less sensible, yet the dilution of the lime-water, in consequence of some of the lime being absorbed by the acid, does; but, I believe, not in any remarkable degree.

There

There is an experiment mentioned by Mr. KIRWAN which, though it cannot be considered as an argument in favour of the generation of fixed air, as he only supposes, without any proof, that fixed air is produced in it, does yet deserve to be taken notice of as a curious experiment. It is, that, if nitrous and common air be mixed over dry quicksilver, the common air is not at all diminished, that is, the bulk of the mixture will be not less than that of the common air employed, until water is admitted, and the mixture agitated for a few minutes. The reason of this in all probability is, that part of the phlogisticated nitrous acid, into which the nitrous air is converted, remains in the state of vapour until condensed by the addition of water. A proof that this is the real case is, that, in this manner of performing the experiment, the red fumes produced on mixing the airs remain visible for some hours, but immediately disappear on the addition of water and agitation.

The most material experiment alledged by Mr. KIRWAN is one of Dr. PRIESTLEY's, in which he obtained fixed air from a mixture of red precipitate and iron filings. This at first seems really a strong argument in favour of the generation of fixed air; for though plumbago, which is known to consist chiefly of that substance, has lately been found to be contained in iron, yet one would not have expected it to be decomposed by the red precipitate, especially when the quantity of pure iron in the filings was much more than sufficient to supply the precipitate with phlogiston. The following experiment, however, shews that it was really decomposed; and that the fixed air obtained was not generated, but only separated by means of this decomposition.

500 grains of red precipitate mixed with 1000 of iron filings yielded, by the assistance of heat, 7800 grain measures of fixed
air,

air, besides 2400 of a mixture of dephlogisticated and inflammable air, but chiefly the latter. The same quantity of iron filings, taken from the same parcel, was then dissolved in diluted oil of vitriol, so as to leave only the plumbago and other impurities. These mixed with 500 grains of the same red precipitate, and treated as before, yielded 9200 grain measures of fixed air, and 4200 of dephlogisticated air, of an indifferent quality, but without any sensible mixture of inflammable air. It appears, therefore, that less fixed air was produced when the red precipitate was mixed with the iron filings in substance, than when mixed only with the plumbago and other impurities; which shews, that its production was not owing to the iron itself, which seems to contain no fixed air, but to the plumbago, which contains a great deal. The reason, in all probability, why less fixed air was produced in the first case than the latter is, that in the former more of the plumbago escaped being decomposed by the red precipitate than in the other. It must be observed, however, that the filings used in this experiment were mixed with about $\frac{1}{17}$ th of their weight of brass, which was not discovered till they were dissolved in the acid, and which makes the experiment less decisive than it would otherwise be. The quantity of fixed air obtained is also much greater than, according to Mr. BERGMAN's experiment, could be yielded by the plumbago usually contained in 1000 grains of iron; so that though the experiment seems to shew that the fixed air was only produced by the decomposition of the impurities in the filings, yet it certainly ought to be repeated in a more accurate manner.

Before I conclude this paper, it may be proper to sum up the state of the argument on this subject. There are five methods of phlogistication considered by me in my paper on air; namely,

namely, first, the calcination of metals, either by themselves or when amalgamated with quicksilver; secondly, the burning of sulphur or phosphorus; thirdly, the mixture of nitrous air; fourthly, the explosion of inflammable air; and, fifthly, the electric spark; and Mr. KIRWAN has not pointed out any other which he considers as unexceptionable. Now the last of these I by no means consider as unexceptionable, as it seems much most likely, that the phlogistication of the air in that experiment is owing to the burning or calcination of some substance contained in the apparatus*. It is true, that I have no proof of it; but there is so much probability in the opinion, that till it is proved to be erroneous, no conclusion can be drawn from such experiments in favour of the generation of fixed air. As to the first method, or the calcination of metals, there is not the least proof that any fixed air is generated, though we certainly have no direct proof of the contrary; nor did I in my paper insinuate that we had. The same thing may be said of the burning of sulphur and phosphorus. As to the mixture of nitrous air, and the combustion of inflammable air, it is proved, that if any fixed air is generated, it is so small as to elude the nicest test we have. It is certain too, that if it had been so much as $\frac{1}{10}$ th of the bulk of the common air employed, it would have been perceived in the first of these methods, and would have been sensible in the second though still less. So that out of the five methods enumerated, it has been shewn, that in two no sensible quantity is generated, and not the least proof has been assigned that any is in two of the

* In the experiment with the litmus I attribute the fixed air to the burning of the litmus, not decomposition, as Mr. KIRWAN represents it, which is a sufficient reason why no fixed air should be found when the experiment is tried with air in which bodies will not burn.

others;

others; and as to the last, good reasons have been assigned for thinking it inconclusive; and therefore the conclusion drawn by me in the above-mentioned paper seems sufficiently justified; namely, that though it is not impossible that fixed air may be generated in some chemical processes, yet it seems certain, that it is not the general effect of phlogisticating air, and that the diminution of common air by phlogistication is by no means owing to the generation or separation of fixed air from it.

XVI. *Reply to Mr. Cavendish's Answer.**By Richard Kirwan, Esq. F. R. S.*

Read March 18, 1784.

I MEAN to trouble the Society but with a very few words in reply to Mr. CAVENDISH's answer, as I consider the greater part of mine to him as still unanswered.

In the first place, he says, that in Mr. LASSONE's experiment the effervescence proceeded not from any fixed air in the alkali, but from the further action of the acid on the zinc from which inflammable air was disengaged. But this could not have happened; for, first, the zinc, instead of being further acted on by the acid, was precipitated according to Mr. LASSONE's own account (p. 8.); and, secondly, the acid was only added by degrees, and undoubtedly would unite to the alkali preferably to the zinc; therefore it was from the alkali, and not from the zinc, that the effervescence arose.

2dly, With regard to the calcination of lead; though in England the smoke and flame may come in contact with the metal, yet in Germany red lead is formed without any communication between them, according to Mr. NOSE, who has given an ample account of this manufactory (p. 86.). Is not lime formed in contact with fuel, flame, and smoke? Mr. MACQUER even thinks it probable, that the contact of flame is hurtful to the production of minium (2 Dict. Chy. 639.). Mr. MONNET made minium by melting lead in a cuppel, in such

such a manner that it was impossible it could come in contact with the least particle of flame or smoke (Mem. Turin. 1769, p. 71.).

Mr. CAVENDISH expresses his surprise at my asserting, that the black powder, which Dr. PRIESTLEY formed out of an amalgam of mercury and lead, was exactly the same as that out of which he had extracted fixed air; but, I think, I have assigned very sufficient reasons for my opinion: how far I was right will best appear by Dr. PRIESTLEY's own letter, in the hands of the Secretary, of which the following is an extract.

“ I certainly imagined the two black powders you write about to be of the same nature, and therefore did not attempt to extract any air from the latter; but immediately on the receipt of your favour of yesterday, I dissolved an ounce of lead in mercury, and expelling it by agitation, put the black powder, which weighed near 12 ounces, into a coated glass retort; then applying heat, I got from it about 20 ounce measures of very pure fixed air, not $\frac{1}{4}$ th of which remained unabsorbed by water.”

Fourthly, it is impossible to attribute the fixed air, produced by the distillation of red precipitate and filings of iron, to the decomposition of the plumbago contained in the iron; for the quantity of fixed air produced in Mr. CAVENDISH's own experiment is more than *twice* the weight of the whole quantity of plumbago contained in the quantity of iron he used, supposing the whole of the plumbago to consist of fixed air, which is not pretended; and more than *eight* times the weight of the quantity of fixed air which plumbago really contains. For Mr. CAVENDISH employed in his experiment 1000 grains of iron and 500 grains of red precipitate, and obtained 7800 grain measures of fixed air, which are equal to 30 cubic inches, and weigh 17 grains. Now 100

A a 2

grains

grains of bar iron contain, according to Mr. BERGMAN, at most, two-tenths of a grain of plumbago; and consequently 1000 grs. of this iron contain but two grains of plumbago; and plumbago, according to Mr. SCHEELÉ, contains but one-third of its weight of fixed air; so that here, supposing the plumbago to be decomposed, we can have at most but seven-tenths of a grain of fixed air, or little more than one cubic inch. If we suppose the filings to be from steel, 1000 grains of steel containing eight of plumbago, we may have about 2,5 of fixed air, or about 1,5 cubic inch, and this is the strongest supposition, and the most favourable to Mr. CAVENDISH. What shall we then say, if we consider that these filings were mixed with copper or brass which contain no plumbago? and, above all, that plumbago cannot be supposed decomposable by red precipitate, since even the nitrous acid cannot decompose it?

5thly, With regard to the power which nitrous selenite has of absorbing fixed air, I must allow the experiments of Mr. CAVENDISH to be just and agreeable to my own; but it only follows, that when fixed air is in its *nascent* state, it is more absorbable. Thus many metallic calces take it from alkalies in its *nascent* state, though in other circumstances they will take none.

Lastly, the permanence of a mixture of nitrous and common air, made over mercury, cannot be attributed to nitrous vapour, as vapour is not elastic in cold; besides, I have often made the mixture without producing any such durable vapour, and this will always happen, when the nitrous air is made from nitrous acid sufficiently diluted.



XVII. *On a Method of describing the relative Positions and Magnitudes of the Fixed Stars; together with some Astronomical Observations.* By the Rev. Francis Wollaston, LL.B. F. R. S.

Read February 5, 1784.

FROM some alterations which have of late years been discovered, in the relative positions and apparent magnitudes of a few of the stars we called fixed, it seems not unreasonable to conclude, that there may be many changes among others of them we little suspect. This thought has led me into a wish, that some method were adopted whereby to detect such motions. The first idea which occurred to me was, to make a proposal to astronomers in general; that each should undertake a *strict* examination of a certain district in the heavens; and, not only by a re-examination of the catalogues hitherto published, but by taking the right ascension and declination of every star in their several allotment, to frame an exact map of it, with a corresponding catalogue; and to communicate their observations to one common centre. This is what I could be glad to see begun. Every astronomer must wish it, and therefore every one should be ready to take his share in it. Such a plan, undertaken with spirit, and carried on gradually with care, would, by the joint labours and emulation of so many astronomers as are now in Europe, produce a celestial Atlas far beyond any thing that has ever yet appeared.

But this would be a work of time, and not within the compass of every one. What I mean now to propose is more immediate; and not out of the reach of any who amuse themselves with viewing the heavenly bodies.

Meridian altitudes and transits can be taken but once in 24 hours; and, though accurate, are therefore tedious. Neither can any re-examination of them be made, but with the same labour as at the first. Equatorial sectors are in the hands of few; and require great skill. Some more general method seemed wanting; to discover variations, which, when detected or only surmised, should be assigned immediately to a more strict investigation.

Turning this in my thoughts, I considered, that the noting down at the time the exact appearance of what one sees, would be far more simple, and shew any alterations in that appearance more readily, than any other method. A Drawing once made would remain, and could be consulted at any future period; and if it were drawn at first with care, a transient review would discover to one, whether any sensible change had taken place since it was last examined. Catalogues, or verbal Descriptions of any kind, could not answer that end so well.

To do this with ease and expedition was then the requisite; and a telescope with a large field, and some proper sub-divisions in it, to direct the eye and assist the judgement, seemed to bid most fair for success.

The following is the method which, after various trials, I have adopted, and think I may now venture to recommend.

To a night-glass, but of DOLLOND's improved construction, which magnifies about six times, and takes in a field of just about as many degrees of a great circle, I have added cross wires, intersecting each other at an angle of 45° . More wires

may be crossed in other directions; but I apprehend these will be found sufficient. This telescope I mount on a polar axis. One coarsely made, and without any divisions on its circle of declination, will answer this purpose, since there is no great occasion for accuracy in that respect: but as the heavenly bodies are more readily followed by an equatorial motion of the telescope, so their relative positions are much more easily discerned when they are looked at constantly as in the same direction. An horizontal motion, except in the meridian, would be apt to mislead the judgement. It is scarcely necessary to add, that the wires must stand so as for one to describe a parallel of the equator nearly. Another will then be a horary circle; and the whole area will be divided into eight equal sectors.

Thus prepared, the telescope is to be pointed to a known star, which is to be brought into the centre or common intersection of all the wires. The relative positions of such other stars as appear within the field, are to be judged-of by the eye: whether at $\frac{1}{2}$, or $\frac{1}{3}$, or $\frac{1}{4}$ from the centre towards the circumference, or *vice versa*; and so with regard to the nearest wire respectively. These, as one sees them, are to be noted down with a black lead pencil upon a large message card held in the hand, upon which a circle, similarly divided, is ready drawn. (One of three inches diameter seems most convenient.) The motion of the heavenly bodies in such a telescope is so slow, and the noting down of the stars so quickly done, that there is most commonly full time for it without moving the telescope. When that is wanted, the principal star is easily brought back again into the centre of the field at pleasure, and the work resumed. After a little practice, it is astonishing how near one can come to the truth in this way: and, though neither the right ascensions nor the declinations are laid down
by

by it, nor the distances between the stars measured; yet their *apparent* situations being preserved in black and white, with the day and year, and hour if thought necessary, written underneath, each card becomes a register of the then appearance of that small portion of the heavens; which is easily re-examined at any time with little more than a transient view; and which yet will shew on the first glance, if there should have happened in it any variation of consequence. It is obvious, that very delicate observations are not to be made in this way.

In order to explain my meaning more fully, a card so marked shall accompany this paper (see tab. V. fig. 1.). What I first happened to pitch upon was the constellation of Corona Borealis, which then fronted one of my windows; and which I have since pursued throughout in this method; making the stars α , β , γ , δ , ϵ , ζ , η , ι , κ , π , ρ , σ , and τ , successively central; together with one or two belonging to Bootes, for the sake of connecting the whole together. These I have transferred since on a sheet of paper, to try how well they would unite into one map; which they have done with very little alteration. A copy of that shall also be laid before this Society (fig. 2.).

My design was, after marking down all such stars as are visible with so small a magnifier, to go over the whole again with another telescope of a higher power, divided in the same way; and after that, with a third and a fourth; so as to comprehend every star I could discern. That would discover smaller changes: but it must be a work of time, if attempted at all. After such a rough map of the constellation is made, the endeavouring to ascertain the right ascensions and declinations of these, may perhaps be advisable in the next place, rather than searching for more.

In

In observing in this way it is manifest, that the places of such stars as happen to be under or very near any one of the wires, must be more to be depended upon, than of what are in the intermediate spaces, especially if towards the edges of the field: so also what are nearest to the centre, because better defined, and more within the reach of one wire or another. For this reason, different stars in the same set must successively be made central, or brought towards one of the wires, where any suspicion arises of a mistake, in order to approach nearer to a certainty: but if the stand of the telescope be tolerably well adjusted and fixed in its place, that is soon done.

In such a glass it is very seldom that light is wanting sufficient to discern the wires. When an illuminator is required, I find, that for this purpose, where you wish to see every small star you can, a piece of card or white paste-board, projecting on one side beyond the tube, and which may be brought forward occasionally, is better than one of any other kind. By cutting across a small segment of the object-glass, it throws a sufficient light down the tube, though a candle is at a great distance; and one may lose sight of that false glare when one pleases, by drawing back the head, and moving the eye a little side-ways, and then one sees the smaller stars just as well as if no illuminator were there.

This then is the method I would recommend to the practical astronomer, for becoming acquainted with the appearance of the stars, and setting a watch over the heavenly motions. After a very few trials, every one would find this easy. And if each person of every rank among astronomers would take a constellation or two under his care, the numbers who could undertake it in this way would compensate for the defects of a plan which cannot aspire at great accuracy. The labour of

it, even at first, is but little. It has cost me more time indeed than I ought commonly to allot to mere amusement; because I had my apparatus to contrive, and several different and fruitless schemes to try, before I could satisfy myself. But a quarter, or at the most half, an hour is generally sufficient for the marking of one pretty full card in this way: and when once the cards are marked, and a general map of the constellation is formed, a little time given to it in a fine evening, to examine whether the stars on such or such a card remain in their former position, is little trouble indeed. Perseverance is most likely to be wanting, and therefore must be determined upon; because, after finding things time after time just as they were, one's hopes of discovering any thing new will slacken. But the different state of the air, or of one's own eye, will frequently occasion a fresh star to become visible, or a small one which had been noted down to seem to have disappeared; and such a mere accident will serve to re-kindle the desire of pursuing it. Besides, if we observe no change after a tolerable interval of assiduous search, we may at any time turn to another constellation: yet ought we never to abandon the former entirely, after having once publicly undertaken it, without giving notice of our so doing.

In the cards or maps, it may be observed, I have not marked the respective sizes of the stars. Nor have I distinguished them in any way, excepting a few of them with BAYER's Greek letters. It was because I have not hitherto satisfied myself how to do it. Some method must be used by every one, to describe to himself what he means; but, in laying any thing before the public, a deference ought to be paid to what has been done by others. The calling any star by a new name would breed confusion: and as I was desirous this should appear before this
Society

Society in its first rude form, that a judgement might be made from it how far such a scheme would promise success, I was unwilling to look into catalogues or capital maps for the numbers or names of the stars, lest I should be tempted to adapt the positions of what I had observed to what I there found set down by more able astronomers. Nothing, therefore, but a hemisphere of SENEX has been consulted, just for knowing how far the constellation is usually reckoned to extend, and what are BAYER's references.

Should this plan meet with approbation, I shall be happy to have proposed it; and will endeavour to forward it in any way that shall be judged proper: or should any other be preferred, which is within the abilities and leisure of one who is engaged in another profession, I shall be as happy to lend what assistance I can to it. My aim is only, to render such observations as I am capable of making, useful to science.

Before I conclude on this head, give me leave to add a few hints. Whether this method be followed, or any other, if a *general plan* be set on foot, whoever undertakes a constellation, or district, should determine to examine it with as great accuracy as he can; yet never be ashamed to let others know of his mistakes. The error of one proves a caution to another. Such a rough sketch, once made, will be found of great use to most of us, in knowing which star next to examine with greater care. He who can do no more than this, will do a useful work by going thus far: and his frequently sweeping over his district in this way, may lead him to a discovery which might escape a more regular astronomer. But whoever can, ought to do more. By degrees the exact positions of every star he has noted down may be ascertained, by the method practised by Mr. DE LA CAILLE in his Southern Hemisphere, or by any

other which shall be esteemed more convenient. Every one, indeed, must use such instruments as he can procure: but assiduity can do more with indifferent ones, than will ever be accomplished with the very best without it. Whatever references are made for one's own convenience, when a map and catalogue are given to the public stock, the old letters and numbers should be retained as far as they go: though yet notice should be taken, where the magnitudes of the stars at present do not appear to correspond with the order in which they have been laid down.

To render this more complete, it were to be wished, that each should give in a copy of his original observations, with an account of the instruments he used; since they ought to be preserved as data from whence his deductions were made, which may then be re-examined at any future time. Yet must it be desired, that no one would trust himself without carrying on his calculations as fast as the observations are made: they will otherwise multiply upon his hands till the labour will dishearten him from attempting it at all. A heap of crude, undigested observations would be an unwelcome present to the public.

Having thus stated this Proposal, I shall leave it to be proceeded upon, or not, as shall be seen proper: And will now only subjoin a List of such occasional observations as I have had opportunity of making, since the last which I communicated to this Society. I find, indeed, that it is much longer than I had apprehended: but as I perceive some astronomers abroad have referred to a few of those which have been honoured with a place in our Transactions, it may be as well to follow it up. An observation retained among one's own private papers I hold to be of little use.

One

One thing let me desire Foreigners to remark : that the registers I gave of the going of my clock were meant only as the relations of a *mere fact* ; that a clock, of such a construction, kept or altered its rate *so* or *so*. They seem to have understood it as an account of a capital clock, by valuing themselves upon some of theirs going better. The time-keepers in most of our Observatories are far more accurate ; but, excepting those of the Royal Observatory at Greenwich, their accuracy is not made public.

Another remark it may also be proper to make ; that, since my former papers, the longitude of this place has been ascertained by comparative observations on the bursting of some rockets, let off on purpose ; which, on a mean of several, turns out to be $19''.02$ in time E. of Greenwich Observatory ; that is, it may hereafter be considered as $19''$, instead of $18''.6$ as I had before calculated it trigonometrically from the bearings.

Observations made at Chislehurst, in Kent, longitude 19" in time East of the Royal Observatory at Greenwich, and latitude 51° 24' 33" North.

Eclipse of the moon, 8 July 30, 1776: observed with a 3½ feet achromatic telescope, and a power magnifying 29 times (that is, a single eye-glass belonging to the day-tube) the aperture of the telescope being reduced to 1½ inches. The night very clear and still.

Apparent time.

h. ' "

The beginning not properly observed.

10 11 31 Grimaldus touched by the shadow.

10 12 49 ——— covered.

10 14 5 Galilæus covered.

10 19 36 Aristarchus covered.

10 26 0 The spot in Kepler bisected.

10 24 25 Schikardus (but 2.) touched.

10 25 52 - - - - - bisected.

10 27 19 - - - - - covered.

10 28 15 Copernicus touched.

10 29 49 - - covered.

10 31 22 Helicon (but 2.) covered.

10 37 9 Plato touched.

10 37 54+ - covered.

10 38 55 Tycho touched.

10 39 39 - - bisected.

10 40 25 - - covered.

Manilius

Apparent time.

h. ' "

10 43 16 Manilius covered.

10 46 51 Menelaus covered.

10 48 5 Dionysius covered.

10 55 4 Censorinus covered.

10 58 57 A point (Promontorium acutum, I believe) touched.

11 0 21 A spot between M. Fœcunditatis and M. Nectaris touched.

11 0 23 M. Crisium touched.

11 3 55 - - - covered.

11 7 57 The eclipse seemingly total.

11 11 11 The moon covers a small star near her south limb.

The star hangs on the limb, before it disappears.

11 28 17 She covers another star a little south of her centre.

This vanishes instantaneously.

These occultations were observed with another power of the same telescope; which is usually reckoned 100, and which I have formerly so called; but which on an accurate examination really magnifies almost 75 times.

The emerfions of these stars were not observed.

12 43 0 I judge the beginning of the emerfion to be about this time; but cannot be certain.

12 48 1 Grimaldus quitted by the shadow.

12 58 25 Aristarchus quitted.

12 59 22 Kepler bisected.

13 0 15 Tycho begins to emerge.

13 1 9 - - bisected.

13 1 53 - - emerges. Till this time I had used the whole aperture (3,6) having forgotten to reduce

I

it,

Apparent time.

h. ' "

it, till the moon's brightness reminded me. Same power as at first ; that is, 29.

- 13 6 51 Copernicus begins to emerge.
 13 7 20 - - - seemingly bisected.
 13 8 19 - - - emerges.
 13 10 27 Helicon emerges.
 13 15 26 Plato begins to emerge.
 13 16 31 - - emerges.
 13 21 30 Manilius emerges.
 13 23 54 Dionysius emerges.
 13 24 57 Menelaus emerges.
 13 29 47 Cenforinus emerges.
 13 31 21 The spot by M. Foecunditatis emerges.
 13 35 31 The point of Prom. Acutum emerges.
 13 37 21 + M. Crisium begins to emerge.
 13 40 26 - - - quitted by the shadow.
 13 42 0 The end of the eclipse.

The air was very clear and still the whole time : the shadow but ill defined. Indeed, it was little more than a penumbra ; the principal spots remaining always visible on the moon's dusky face.

Eclipse of the sun & June 24, 1778 : observed with a $3\frac{1}{2}$ feet achromatic telescope magnifying 75 times. The aperture reduced to two inches, to prevent breaking the smoked glasses.

- 3 41 33,5 Beginning. I suspect the minute to be mistaken, and that it should be 3 h. 40' 33'',5. The first impression

Apparent time.

h. ' "

impreſſion could not be 2", I believe not 1", before I obſerved it.

5 25 24 End. An undulation on the ſun's limb; but the obſervation pretty good.

Eclipse of the moon 8 November 23, 1779: obſerved with the ſame telescope, magnifying 75 times. The aperture reduced to two inches. Night clear and froſty. No wind.

The beginning not aſcertained.

6 13 19 Grimaldus touched by the ſhadow.

6 13 28 - - - covered.

6 17 29 Ariſtarchus covered.

6 20 46 Kepler biſected.

6 23 40 M. Humorū touched.

6 27 47 Helicon covered.

6 28 40 Copernicus and Timocharis both biſected.

6 29 57 M. Humorū covered.

6 33 50 Plato touched.

6 34 27 - - covered.

6 41 52 Tycho touched.

6 43 8 - - covered.

6 47 11 Plinius (but 2.) covered.

6 59 1 M. Criſum touched.

7 3 16 - - - covered.

7 7 31 The eclipse total.

8 46 23 Moon's edge begins to emerge.

8 51 14 Grimaldus begins.

8 52 1 - - - emerges.

A haze comes on.

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C c

Kepler

Apparent time.

h. ' "

9 2 23 :: Kepler bisected. This not clearly seen.

9 11 41 Plato begins to emerge.

9 12 35 - - emerges.

9 13 46 Tycho emerged.

The haze comes on again too much for the observation to be pursued any farther.

Eclipse of the sun & Oct. 16, 1781 : observed with the same telescope and magnifying power.

The beginning not visible ; sun too low.

20 22 13,5 The end. Good.

Eclipse of the Moon & Sept. 10, 1783 : observed with the same telescope, viz. $3\frac{1}{2}$ feet achromatic, with the aperture reduced to two inches ; but with a small magnifying power of 36 times, which I had made by Mr. DOLLOND for these observations, and which I found very convenient. Night a little hazy, but pretty favourable.

9 33 0 A duskiness comes on the moon.

9 45 35 The beginning of the shadow, I believe.

9 47 20 A haziness obscures the moon.

9 50 55 Aristarchus covered.

9 52 20 Kepler covered. So it is set down ; but I do not recollect what I meant by this ; whether it might not be only the spot in the centre, so that it might more properly be called bisected.

Gassendus

Apparent time.

h. ' "

9 57 57 Gassendus covered. I suspect the minute here; and that it should be 56' 57".

9 59 41 Heraclides covered.

10 1 42 Copernicus touched.

10 3 5 - - - covered.

10 3 26 Helicon covered.

10 4 12 Bulialdus covered.

10 8 0 A hazinefs again.

10 8 57 Plato covered.

10 15 30 Manilius covered.

10 15 54 Tycho touched.

10 17 5 :: - - covered. This doubtful.

10 19 10 Menelaus covered.

10 21 38 Dionysius covered.

10 22 40 Plinius covered,

A hazinefs again.

10 28 25 Cenforinus covered.

10 34 34 M. Crisium touched.

10 39 45 - - - covered.

10 46 34 Total darknefs, as I judged it.

At 10 h. 41' the moon had grown reddish, and the eclipsed part become more visible than before.

After some time, during the total darknefs, the moon was barely to be seen. In general, about the centre, it was darker than towards the circumference, which was ill-defined. About

12 0 0 The eastern limb became more visible, and better defined.

C c 2

The

Apparent time.

h. ' "

- 12 14 0 The light spreads a great way over the moon from
that side towards the centre, extending about
two-thirds of her circumference (see fig. 3.)
- 12 23 0 The moon seems beginning to emerge.
- 12 25 0 Emerfion certainly has begun.
- 12 28 21 Grimaldus emerged.
- 12 31 40 Galileus emerged.
- 12 33 52 Ariftarchus emerged.
- 12 37 26 Kepler (but 2. this as before).
- 12 39 36 Heraclides emerged.
- 12 42 56 Helicon emerged.
- 12 45 52 Copernicus emerged entirely.
- 12 47 22 Plato begins to emerge.
- 12 47 58 - - emerges.
- 12 48 30 Tycho begins to emerge.
- 12 49 58 - - emerges.
- 12 58 8 Manilius emerges.
- 13 1 40 Menelaus emerges.
- 13 3 18 Dionyfius emerges.
- 13 5 40 Plinius emerges.
- 13 11 22 Cenforinus (but 2.) emerges.
- 13 16 35 M. Crifium begins to emerge.
- 13 20 53 - - - emerges.
- 13 25 38 The fhadow quits the moon near Langrenus, be-
tween that and M. Crifium. The dufkinefs does
not leave the moon till fome time afterwards, but
I did not wait to obferve it.

The moon was darker during the eclipfe than ufual;
but the air was not clear enough for any occulta-
tions of ftars to be obferved.

Transit

Transit of Mercury over the sun's disk 3 Nov. 12, 1782: observed with the same telescope, and a power of 75 times. The aperture reduced to two inches.

Apparent time.

h. ' "

- 2 51 49 First impression observed. It could not be 2'' sooner.
 2 54 57 Thread of light completed; but seen through clouds. The planet seemed to hang on the sun's limb 30'' at least.
 4 6 0 Through a break in the clouds, of short duration, & seemed to have quitted the sun; but indeed the clouds were very unfavourable the whole time.

Occultation of Saturn by the moon, 1 February 18, 1775: observed with the same telescope; and, I believe, the same power, with the whole aperture of the object-glass 3.6 inches; but, I perceive, I have not set down these particulars.

- 9 5 39 Præc. ansa of the ring im.
 9 6 9 Præc. limb of the planet im.
 Subsequent limb not set down.
 9 6 48 Subsequent ansa im.
 The moon low at these immersions, and much undulation. The emersions lost by looking at a wrong part of the moon's disk, except
 10 1 7 Subsequent ansa emerges.
 Night very clear; but the observation on the whole imperfect.

Occultations

198 *Mr. WOLLASTON'S Astronomical Observations.*

Occultations of stars by the moon: observed with the same telescope, and a power of 75 times, with the whole aperture of the object-glass.

		Apparent time.			
		h.	'	"	
1775.					
♂ Aug. 1.	♂ γ Virginis	7	48	17	Both stars visible when a cloud covered them.
		7	49	20	A short break; only one star visible.
		7	52	15	Another break; but before this the second star was immersed.
		8	48	58,5	First * em. good.
		8	49	6,5	Second * em. good.
	♂ a bright * N of γ Virginis }	8	54	13	Im. good.
					Em. not till the moon was too low.
♂ Dec. 12.	♂ Regulus	10	5	46	Em. very good, though the moon low.
1776.					
⊙ June 30.	⊙ ι ad μ ♄	9	3	49	Im good; some flying clouds.
		10	6	38	Em.; perhaps sooner.
1777.					
♂ Aug. 23.	♂ μ Ceti	10	41	17	Im.: the moon low; night clear and still.
		11	32	10	Em.
♂ Nov. 15.	♂ ι ad δ Tauri				Im. not seen; undulation too great.
		7	22	56	Em. pretty good.
⊙ Nov. 16.	♂ ζ Tauri	11	17	1,5	Im. good.
		12	23	28	Em. good.
					{ These were observed with a power of 67 times, and an oblique speculum.
1783.					
♀ May 16.	♂ π Scorpii	11	21	49	Im. }
		12	31	49,5	Em. }
					} Night clear and still; the observations good.
♂ Jul 10.	♂ π Scorpii				Im. not seen for clouds.
		8	43	56	Em.; it might be 1" or 2" sooner; the moon's edge ill defined.
♂ Dec. 30.	♂ δ Piscium	8	3	13	Im. dark limb, very good.
		9	8	30	Em. good. It could not be above 1" sooner, if that. Night very clear and still; hard frost; therm. 13° $\frac{1}{2}$.

Eclipses of Jupiter's satellites : observed with the same telescope and power (that is, 75 times ; called usually 100) and whole aperture.

Apparent time.

			h.	'	"	
1775.						
♀ Sept. 8.	1 Sat.	11	33	14		Im. flying clouds ; observation doubtful.
☉ Oct. 1.	1 Sat.	11	51	1		Im. good ; unless the minute be mistaken.
♂ Nov. 2.	1 Sat.	8	28	2		Im. good.
♂ 16.	2 Sat.	9	0	13		Im. pretty good ; air clear, but a cold in my eyes rendered the observation not satisfactory.
♂ Dec. 18.	1 Sat.	10	45	48		Em. good.
	2 Sat.	11	2	0		Em. pretty good.
♂ 27.	1 Sat.	7	3	48		Em. good.
1776.						
☉ Nov. 17.	3 Sat.	9	38	48,5		Im. ; a scintillation for some seconds before it quite disappeared.
1778.						
♂ May 21.	1 Sat.	9	9	38		Em. good.
	2 Sat.	10	10	±		Em. so near the first satellite ; as scarcely to be distinguishable from it for some minutes.
♂ June 11.	4 Sat.	9	52	4		Im. good for the fourth satellite, yet visible by fits for some seconds longer.
♂ 13.	1 Sat.	9	19	6		Em. pretty good.
1779.						
♂ Mar. 9.	1 Sat.	6	59	19		Im. ; that is, this was the last of my seeing it ; but, though the night was clear, the satellite was too near Jupiter for the observation to be satisfactory.
♂ May 22.	2 Sat.	11	5	54		Em. good.
1781.						
♂ May 24.	1 Sat.	10	3	31		Em. very good.
♂ 31.	1 Sat.	11	57	35		Em. pretty good.
♂ June 16.	1 Sat.	10	13	13		Em. ; clouds, but pretty good.
1782.						
♂ July 20.	3 Sat.	9	6	42		Em. good.
	2 Sat.	11	30	30		Em. good.

Emerſion

Apparent time.

h. ' "

☉ July 21. 1 Sat. 9 39 50 Emerfion; windy; but good.
 ♃ Aug. 29. 1 Sat. 8 20 15,5 Em.
 ♀ 30. 4 Sat. 8 52 19 Em.; fatellite feen then, but not diftinct for
 fome time.

1783.

♂ July 8. 1 Sat. 12 14 13 Im. pretty good.
 ♃ Aug. 2. 1 Sat. 9 10 31,5 Em. good.
 ♃ 25. 1 Sat. 9 28 54 Em.
 ♀ Sept. 26 1 Sat. 6 19 44 Em. pretty good, but twilight ftrong.
 ♂ 30. 3 Sat. 10 3 24 Im. It was vifible only by fits for the laft 8".
 Jupiter near a tree.
 ♀ Oct. 3. 1 Sat. 8 18 0 Em. pretty good; but the moon below Jupiter.
 ☉ 26. 1 Sat. 8 39 17 Em. Jupiter low and near a tree; great undu-
 lation.

EXPLANATION OF THE FIGURES IN TAB. V.

- Fig. 1. a Cor. Bor. & Aug. 6, 1783, per night-glafs. The * marked Sept. 24. was not obferved till that night, but has continued fince, and was only overlooked at firft.
- Fig. 2. A map of 107 ftars, befides thofe marked by BAYER, in the conftellation of Corona Borealis, or the Northern Crown; together with a part of Bootes: laid down from obfervations made 1783 with a night-glafs furnifhed with crofs-wires; as their relative pofitions were eftimated by the eye.
- Fig. 3. The moon as ſhe appeared (inverted) & Sept. 10, 1783, about a quarter of an hour before ſhe began to emerge from total darknefs.



Fig. 1.

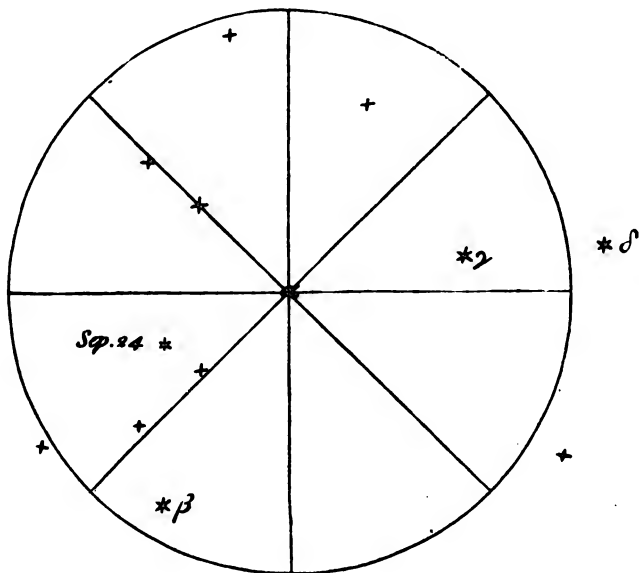
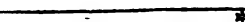


Fig. 3.

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XVIII. *An Account of some late fiery Meteors; with Observations. In a Letter from Charles Blagden, M. D. Physician to the Army, Sec. R. S. to Sir Joseph Banks, Bart. P. R. S.*

Read February 19, 1784.

TO SIR JOSEPH BANKS, BART. P. R. S.

DEAR SIR,

FROM the papers you were so good as to put into my hands, together with such other information as I could procure, the following account of the two most remarkable of the late meteors is collected. I am sensible, that it is in many respects very imperfect; yet still it gives a more satisfactory idea of the phænomena than can well be acquired from the relation of any single observer, and therefore may not be disagreeable to the learned Society over which you so worthily preside, if no more perfect account shall previously have been laid before them.

These meteors were of the kind known to the ancients by the names of *Λαμπαδες*, *Πιθοι*, Bolides, Faces, Globi, &c. from particular differences in their shape and appearance, and sometimes, I believe, under the general term of Comets*;

Philosophical

* ARISTOTLE's remark, that all the comets seen among them disappeared without setting, *Ἀστὴρ δὲ καὶ ἡμᾶς οὐ μὲντοι (Κομήτης) αὐτὸν διδόντες φανισθῆναι αὐτὸν ἔτι τὰ ἑσπέρια τοῦ* (Meteor. lib. I. c. 6.), seems scarcely applicable but to transitory meteors; and many other expressions to the same purpose occur in that author,

Philosophical Transactions they are called indiscriminately fire-balls or fiery meteors; and names of a similar import have been applied to them in the different languages of Europe.

The most material circumstances observed of such meteors may be brought under the following heads: 1. Their general appearance. 2. Their path. 3. Their shape or figure. 4. Their light and colours. 5. Their height. 6. Their noise. 7. Their size. 8. Their duration. 9. Their velocity.

I shall begin with the first of these meteors, that which was seen on the 18th of August.

§ 1. Its general appearance in these parts of Great Britain was that of a luminous ball, which rose in the N.N.W. nearly round, became elliptical and gradually assumed a tail as it ascended, and in a certain part of its course seemed to undergo a remarkable change compared to bursting; after which it proceeded no longer as an entire mass, but was apparently divided into a great number or a cluster of balls, some larger than the others, and all carrying a tail or leaving a train behind; under this form it continued its course with a nearly equable motion, dropping or casting off sparks, and yielding a prodigious light, which illuminated all objects to a surprising degree; till having passed the east, and verging considerably to the southward, it gradually descended, and at length was lost out of sight. The

PLIN^y, *Sen.* SEVERA himself, though he conceived distinctly enough the difference between comets and fiery meteors, yet evidently did not know where to draw the line (compare lib. I. and VII. Quæst. Natur.). Even in modern times, these meteors have struck spectators at first as comets (Fritzes Medizinische Annalen, vol. I. p. 77.); nay, expert astronomers, as appears by a letter from NATHANIEL PIGOTT, Esq. Fr. R. S. lately read before the Royal Society. See also Mem. de l'Ac. des Sciences 1771, p. 688. I have insisted the more on a subject apparently of so little consequence, in order to account for the strange opinions of the ancients respecting comets, which, I think, proceeded chiefly from confounding them with these fiery meteors.

time

time of its appearance was 9 h. 16' P.M. mean time of the meridian of London, and it continued visible about half a minute.

§ 2. How far north the meteor may have begun I have no materials to determine with precision; but, as it was seen in Shetland, and at sea between the Lewes and Fort William, and appeared to persons at Aberdeen and Blair in Athol ascending from the northward, and to an observer in Edinburgh as rising like the planet Mars, there can be little doubt but its course commenced beyond the farthest extremity of this island, somewhere over the northern ocean. General MURRAY F. R. S. being then at Athol House, saw it pass over his head as nearly vertical as he could judge, tracing it from about 45° of elevation north-north-westward to 30° or 20° south-south-eastward, where a range of buildings intercepted it from his view. From near the zenith of Athol House, it passed on a little westward of Perth, and probably a little eastward of Edinburgh; and continuing its progress over the south of Scotland, and the western parts of Northumberland and the Bishopric of Durham, proceeded almost through the middle of Yorkshire, leaving the capital of that county somewhat to the eastward. Hitherto its path was as nearly S.S.E. as can be ascertained; but somewhere near the borders of Yorkshire, or in Lincolnshire, it appears to have gradually deviated to the eastward, and in the course of that deviation to have suffered the remarkable change already noticed under the denomination of bursting. After this division, the compact cluster of smaller meteors seems to have moved for some time almost S.E. thus traversing Cambridgeshire and perhaps the western confines of Suffolk; but gradually recovering its original direction, it proceeded over Essex and the Straits of Dover, entering the continent probably not

far from Dunkirk, where, as well as at Calais and Ostend, it was thought to be vertical. Afterwards it was seen at Brussels, Paris, and Nuits in Burgundy *, still holding on its course to the southward; nay, I have met with an intimation, though of doubtful authority †, that it was perceived at Rome. Our information of its progress over the continent is, indeed, very defective and obscure; nevertheless, I think, we have sufficient proof that it traversed in all 13 or 14 degrees of latitude, describing a track of 1000 miles at least over the surface of the earth; a length of course far exceeding the utmost that has been hitherto ascertained of any similar phenomenon.

To adduce the different accounts from which this path is determined, would not only be insufferably tedious, but contrary to the intention of this letter, which is to give a summary view of the whole. They are contained partly in letters, and partly in the different news-papers of England and Scotland, most of which have been perused for this purpose. The information derived from the news-papers, however incorrect in the detail, is brought to some degree of certainty by the check of comparing them with one another; and their frequent publication in most places of consequence in this island, procures us advantages on the occasion of such extraordinary phenomena, not enjoyed in former ages, nor even now, to the like extent, in any other part of the world.

It seems scarcely more interesting to trace the path of these bodies with minute precision, than it would be to mark the progress of a cloud's shadow upon the ground; but it is of consequence to their theory to ascertain well the direction of their course; and their deviations from a straight line, as implying some particular cause, should be carefully noticed. B

* Journ. de Paris, Août 24, 1783.

† PARKER's General Advertiser, October 7, 1783.

have

have ventured to ascribe such a deviation to this meteor, from the concurrent testimony of many observers, who speak in the plainest terms of a manifest change in its course about the time it was seen to burst; and their evidence is confirmed by drawing a line S.S.E. from that part of Scotland to which the meteor was vertical, for such a line is found to correspond with its path as far as Yorkshire, but in the southern parts of the kingdom falls a great deal too much to the westward. That it afterwards resumed its former course is rendered probable from the testimony of the observers in Kent, who almost uniformly mention its disappearance in the S.S.E. as well as from the remarks made by several persons near the metropolis, that when it attained its greatest elevation, it bore but one or two points to the northward of east.

§ 3. This meteor was described by most spectators under three different forms, and is thus represented by Mr. SANDBY in his beautiful Drawing*; but the two first of those do not imply any real variation in its shape, depending only on a difference in the point of view. Accordingly, in the first part of its course over Scotland, it was seen to have a tail, and is thus described by General MURRAY when it passed Athol House. Two causes concur in this deception; first, the fore-shortening, and even occultation, of the tail, when the object is seen nearly in front; and, secondly, that the light of most part of the tail is of so inferior a kind, as to be difficultly perceived at a great distance, especially when the eye is dazzled by the overpowering brilliancy of the body. The length and shape of the tail, however, were perpetually varying; nor did the body continue always of the same magnitude and figure, but was sometimes round, at other times elliptical, with a blunt or

* Since engraved. See also the figures tab. IV. of this volume.

pointed

pointed protuberance behind. From such changes of figure in this and other meteors it is, that they have been compared to columns or pyramids of fire, comets, barrels, bottles, flasks, paper-kites, trumpets, tadpoles, glass-drops, quoits, torches, javellins, goats, and many similar objects; whence the multifarious appellations given to them by the ancients were borrowed.

Respecting the tails of meteors, it is here necessary to distinguish between two different parts of which they consist. The brightest portion seems to be of the same nature as the body, and indeed an elongation of the matter composing it; but the other, and that commonly the largest portion, might more properly be called the train, appearing to be a matter left behind after the meteor has passed; it is far less luminous than the former part, and often only of a dull or dusky red colour. A similar train or streak is not unfrequently left by one of the common falling stars, especially of the brighter sort; and vestiges of it sometimes remain for several minutes. It often happens, that even the large fire-balls have no other tail but this train, and ours of the 8th of August appeared at times to be in that state; its tail was likewise thought by some spectators to be spiral.

Under this changeable form, but still as a single body, it proceeded regularly till a certain period, when expanding with a great increase of light, it separated into a cluster of smaller bodies or ovals, each extended into a tail and producing a train. At the same time a great number of sparks appeared to issue from it in various directions, but mostly downward, some of which were so bright as also to leave a small train. Most fire-balls have suffered a bursting or explosion of this kind; but in general they have been thought to disappear immediately afterwards.

inwards. *THE*, however, continued its course, becoming more compact, or perhaps re-uniting, and seems to have undergone other similar explosions before it left our island, and again upon the continent*. The different accounts tend to shew, that its first separation or bursting happened somewhere over Lincolnshire, perhaps near the commencement of the fens. Many observers did not get sight of it till after this period, and therefore never describe it as a single ball. There appears to be some deception, in consequence of which spectators are led to believe, that a meteor is extinguished by these explosions; for the same opinion was formed of this in several parts of its course, though we have such decisive evidence of its continued progress; whether it be that the meteors really become more dull for a time immediately after their explosion, or merely appear so on account of the greater preceding light, since they are always described as being most luminous the instant they burst.

It is observable, that the great change in this meteor corresponds with the period in which it suffered a deviation from its course, as if there was some connexion between those two circumstances; and there are traces of something of the same kind having happened to other meteors. If the explosion be any sort of effort, we cannot wonder that the body should be moved by it from a straight line; but on the other hand it seems equally probable, that if the meteor be forced, by any cause, to change its direction, the consequence should be a division or separation of its parts.

§ 4. Nothing relative to these meteors strikes the beholders with so much astonishment as the excessive light they afford,

* For another instance of repeated explosions consult *Mém. de l'Ac. des Scienc.* 1756, P. 23.

sufficient

sufficient to render very minute objects visible upon the ground in the darkest night, and larger ones to the distance of many miles from the eye. The illumination is often so great as totally to obliterate the stars, to make the moon look dull, and even to affect the spectators like the sun itself; nay, there are many instances in which such meteors have made a splendid appearance in full sun-shine. The colour of their light is various and changeable, but generally of a bluish cast, which makes it appear remarkably white. A curious effect of this was observed at Brussels the 18th of August, that whilst the meteor was passing, "the moon appeared quite red, but soon recovered its natural light*." The brightness alone of the meteor is not sufficient to explain this, for the moon does not appear red when seen by day; but it must have depended on the the contrast of colour, and shews how large a proportion of blue rays enters into the composition of that light, which could make even the *silver* moon appear to have excess of red. Prismatic colours were also observed in the body, tail, and sparks of this meteor, variously by different persons; some compared them to the hues of gems. The moment of its greatest brightness seems to have been when it burst the first time; but it continued long to be more luminous after that period, than it was before.

The body of the fire-ball, even before it burst, did not appear of an uniform substance or brightness, but consisted of lucid and dull parts, which were perpetually changing their respective positions; so that the whole effect was to some eyes like an internal agitation or boiling of the matter, and to others like moving chasms or apertures. Similar expressions

* From a letter of the Abbé MANN's, Director of the Academy at Brussels, to Sir JOSEPH BANKS, Bart. P. R. S.

have

have been used in the description of former meteors. The luminous substance was compared to burning brimstone or spirits, Chinese fire, the stars of a rocket, a pellucid ball or bubble of fire, liquid pearl, lightning and electrical fire; few persons fancied it to be solid, especially when it came near the zenith. Different spectators observed the light of the meteor to suffer at times a sudden diminution and revival, which produced an appearance as of successive inflammation; but might, in some cases at least, be owing to the interposition of small clouds in its path.

§ 5. When, in consequence of a more accurate attention to natural philosophy, such observations were first made upon fire-balls as determined their height, the computers were with reason surprised to find them moving in a region so far above that of the clouds and other familiar meteors of our atmosphere; especially as to every uninformed spectator they appear extremely near, or as if bursting over his head, a natural effect of their great light when seen without intervening objects. Their real height is to be collected from observations made at distant stations, which, for the greatest accuracy, ought to be so situated, that the line joining them may cut the path of the meteor at right-angles, and that, at its greatest elevation, it may appear from both of them about 45° above the horizon, on opposite sides of the zenith. Also two stations on the same side of its path, if the least angle of elevation be not very small, and the difference between that and the greatest angle be considerable, are by no means to be rejected. But little reliance can be placed upon observations of a meteor's altitude at any supposed period of its course, such as the moment of its bursting; because those changes are seldom so in-

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stantaneous, or seen so much alike by different spectators, as to be marked with sufficient certainty.

Even in proper stations it rarely happens, that the angle of elevation can be observed with that degree of accuracy, which is necessary for any certain determination of the height. An estimate by the eye is doubtful, not only on account of the flattened curve the sky seems to describe, for which the most experienced observers scarcely ever make a just allowance, but likewise of the emotion produced by such an unexpected, magnificent, and perhaps alarming spectacle, which renders it almost impossible to be quite collected. Therefore, unless an observation be checked by means of a house, tree, or some fixed body, along which the meteor was found to range, it must be received as uncertain. By night the stars afford excellent marks, especially if the time be known with exactness; the brighter meteors, indeed, render these faint lights invisible for the moment, but here we derive an eminent advantage from the train, which remains after the meteor is gone, and delineates perfectly its track through the heavens. If no such marks have been taken, the expedient of endeavouring to recollect the part of the sky where it passed, and ascertaining that height with a quadrant, may often be useful; but there are many men of such a turn of mind, that the original impression made upon them will be totally perverted by their own subsequent reflexions and the remarks of others; in which case such an application of instruments is likely to give a result farther from the truth, than their first immediate judgement, however vague and hazarded.

I am sorry to add, that most of the observations in my possession of the meteor which appeared the 18th of August, give its altitude by estimation only; yet I hope their correspondence
I with

with one another will gain them a degree of credit, to which, if single, they would not be entitled.

1. In a letter from Perth in Scotland it is said, that “a gentleman, who has a very good eye, observed the meteor pass about 6° to the westward of the zenith;” and a Professor in one of the Universities, being at Ardoch on the banks of the Tweed, about two miles below Dunbarton, judged it to have “at least 45° of elevation above the horizon.” These altitudes would make its real height 57 statute miles.

2. At St. Andrew’s in Scotland, “it was not quite vertical, but according to some was 20° or 25° from the zenith, according to others not so much.” Taking the greatest of these distances as nearest the truth, since we are usually led to estimate altitudes greater than they really are, this observation, calculated with that of Ardoch, gives 60 miles for the height.

For the communication of these observations, collected by his friends, I am indebted to General MELVILL F. R. S.

At Edinburgh the meteor passed very near the zenith, in which case a deviation of a few degrees is scarcely perceptible to a common eye.

The rev. Mr. WATSON of Whitby, in a letter to Lord MULGRAVE V. P. R. S. is very confident, that the greatest altitude of the meteor, which passed to the westward of his zenith, was 60° . Mr. EDGEWORTH F. R. S. in his letter to you, Sir, states its elevation at Edgeworth’s-Town near Mullingar, in Ireland, as 10° or 12° above the eastern horizon. These observations, calculated strictly from the latitudes and longitudes with the allowance for the curvature of the earth, as indeed were all the rest where the difference would be sensible, give 57 miles for the height of the meteor.

4. In the Morning Chronicle of Sept. 19. is inserted a letter from Newton Ardes, 7 miles east of Belfast, in Ireland, corresponding so well with Mr. EDGEWORTH's in the description of the meteor, as to appear very good authority. The altitude is there given as 16° , whence a height of 58 miles with the observation at Whitby.

5. Mr. MORE, Secretary to the Society for the encouragement of Arts, Manufactures, and Commerce, saw the meteor as he was riding about three miles S.W. of Broseley in Shropshire, and judged it to be elevated 35° . By a perpendicular drawn from this spot to its supposed path in Lincolnshire, its height came out 59 miles.

6. The altitude of 25° determined at Windsor I take to be one of those on which most reliance can be placed, because the gentlemen present, two of them Fellows of the Royal Society, were remarkably well qualified for such an estimation. The letter you received, Sir, from Professor ALLAMAND of Leyden, mentions that the meteor was seen there about 30° above the horizon, and the terms in which it is described in the Dutch news-papers * agree with this account. Its height hence calculated appears to be 58 miles.

7. Mr. THOMAS SQUIRE, of Folkestone, observed the meteor over his house, as he was in the posture of leaning back against a hedge; he afterwards tried "its ranging with the roof by a quadrant, and found it $68^{\circ}\frac{1}{2}$ above the horizon." Reducing this observation to the perpendicular dropped from Windsor on the path of the meteor, its height comes out 54 or 55 miles. Mr. SQUIRE's altitude, determined by a fixed object, is confirmed by the estimate of several persons at Ramsgate.

* *Amsterdamsche Courant*, Aug. 28, 1783.

8. The meteor was seen by Mr. STEEVENS F. R. S. at Hampstead near London, moving along over the top of a row of trees. Mr. CAVENDISH F. R. S. having taken the altitude of these trees with a quadrant, found that of the highest, as seen from the part of the garden-walk opposite to it, to be 33° ; which corresponds very well with the other observations, and consequently gives the same height for the meteor. Mr. STEEVENS kept his eye upon it constantly, whilst he passed briskly along the walk.

This agreement of the different altitudes is nearer than could be expected; yet I know of no contradictory observations of any authority, except some made near Plymouth and in Cornwall, where the meteor being pretty near the horizon, its altitude, as will commonly happen in such cases, is given too great. The effect of this, however, would be to shew, that the meteor was higher; and therefore, I think, we may safely conclude, that it must have been more than 50 miles above the surface of the earth, in a region where the air is at least 30000 times rarer than here below.

Contrary to what has been asserted of most other fire-balls, this of the 18th of August appears by the preceding observations to have kept on in a parallel course, without any descent or approach toward the earth. It may be much questioned, whether such a descent has been proved in any former instance. The meteor described by Sir JOHN PRINGLE has been cited as the most certain example; but any person who carefully examines the observations themselves, as stated in the 51st volume of the Philosophical Transactions, will find them totally inadequate for such a conclusion; its height seems to me determined only in one part of its course, between Island-Bridge
and

and Ancram, and was there from 48 to 50 miles *. M. LE ROY supposes the fire-ball seen July 17, 1771, to have been 54 miles high when it began, and 27 at its explosion †; but does not give the facts on which his calculation is founded.

Every philosopher must be struck with the agreement of these meteors in their distance from the earth, just beyond the limits of our crepuscular atmosphere.

§ 6. That a report was heard some time after the meteor of the 18th of August had disappeared, is a fact which rests upon the testimony of too many witnesses to be controverted, and is, besides, conformable to what has been observed in most other instances. In general it was compared to the falling of some heavy body in a room above stairs, or to the discharge of one or more large cannon at a distance. That rattling noise, like a volley of small arms, which has been remarked after other meteors, does not seem to have been heard on this occasion. From a comparison of the different accounts, it appears as if the report was loudest in Lincolnshire and the adjacent countries, and again in the eastern parts of Kent; in the intermediate places it was so indistinct as generally not to have been noticed, and all observers of credit in Scotland deny that they heard any thing of the sort. If, therefore, this report be connected with the bursting of the meteor, I should be inclined to suppose, that sound was produced two separate times, namely at the first explosion over Lincolnshire, and again when it seemed to burst soon after entering the continent. Ingenious men have availed themselves of this sound, to calculate the distance and height of meteors; and the exactness attained by this method, in the computation of the late fire-ball from the report heard at

* Phil. Transf. vol. LI. p. 241. and 274.

† Mem. de l' Acad. des Scienc. 1771, p. 676.

Windsor,

Windfor *, is very remarkable ; but in general the accounts disagreed so much, that it would have been impossible to conclude any thing from them. Perhaps too the method itself is less certain than has been thought ; for as the propagation of sound, and with intensity too, in air rarefied 30000 times, presents great difficulties in theory, though it may be in some measure explicable from the vast bulk of the meteor, and the large quantity of this rare air it may therefore displace by a sudden expansion ; I think it not improbable, that some hitherto unperceived circumstance comes into play, by which the whole effect may be modified : for instance, if matter belonging to the meteor itself be what conveys the sound to our lower atmosphere, it may either admit sound to be propagated through it at a different rate than through common air, or it may move much faster than sound travels, as the entire meteor certainly does, and carry on the sonoric vibrations with it. Moreover, we cannot be sure what is the velocity of sound in air so much rarer than where our experiments have been made. For these reasons, while we distrust calculations of meteors founded on the progress of sound, we should be particularly careful to note down the intervals, and all the circumstances, as they may lead to very curious discoveries. The effect of the noise is, frequently, to produce such a shaking of the doors, windows, and the whole house, as is mistaken for an earthquake.

Besides the report as of explosions which was heard *after* the meteor, another sort of sound was said to *attend* it, more doubtful in its nature, and less established by evidence ; I mean, a kind of hissing, whizzing, or crackling, as it passed along. That sound should be conveyed to us in an instant from a body above 50 miles distant, appears so irreconcilable to all we know of philosophy, that perhaps we should be justified in

* See p. 111. of this volume.

imputing

imputing the whole to an affrighted imagination, or an illusion produced by the fancied analogy of fireworks. The testimony in support of it is, however, so considerable, on the occasion of this as well as former meteors, that I cannot venture to reject it, however improbable it may be thought, but would leave it as a point to be cleared up by future observers.

§ 7. To determine the bulk of the fire-ball, we must not only have calculated its distance, but also know the angle under which it appeared. For this purpose the moon is the usual term of comparison; but as it was thought, at very different distances, to present a disk equal to that luminary's, and the same expressions have been applied to most preceding fire-balls, I conceive this estimation rather to be a general effect of the strong impression produced by such splendid objects on the mind, than to convey any determinate idea of their size. However, if we suppose its transverse diameter to have subtended an angle of $30'$ when it passed over the zenith, which probably is not very wide of the truth, and that it was 50 miles high, it must have been almost half a mile across. The tail sometimes appeared 10 or 12 times longer than the body; but most of this was train, and the real elongation behind seems seldom to have exceeded twice or thrice its transverse diameter, consequently was between one and two miles long. Now if the cubical contents be considered, for it appeared equally round and full in all directions, such an enormous mass, moving with extreme velocity, affords just matter of astonishment.

§ 8. The duration of the meteor is very differently stated, partly because some observers had it in view a much longer time than others, and partly because they formed different judgements of the time. Those who saw least of it seem to have perceived its illumination about ten seconds, and those
who

who saw most of it about a minute: hence the various accounts may in some measure be reconciled. Mr. HERSCHEL F. R. S. at Windfor, must have kept it in sight long after other observers had thought it extinct: for though, probably, he did not see the beginning, as it never appeared to him like a single ball, he watched it as much as "forty or forty-five seconds, the last twenty or twenty-five of which it remained almost in one situation, within a few degrees of the horizon." This confirms the foreign accounts of its long progress to the southward.

As scarcely any one had sufficient presence of mind to minute the time by his watch, the periods given for its duration are mostly by guess. To correct this rude conjecture, it has been proposed, that the observer should endeavour to pass over the time in his own mind as well as he can by recollection, whilst another person silently marks the seconds with a watch. This may do something, but still leaves the matter very uncertain, as the nature of the emotion felt by the spectator while it was passing will cause the impression of a longer or shorter time to be left upon his mind; and the formal process of recollection is so tedious, that I believe the duration will in this way generally be made too short. Mr. HERSCHEL, at my request, was so good as to act over his observation, with the positions and gestures he was obliged to employ; and this seems likely to come nearer the truth than a simple effort of the mind at recollection. But the surest method would be, to repeat any uniform action in which the spectator might have been engaged at the time; as, for instance, to walk over the same space of ground that he passed while the meteor was in sight.

§ 9. From the apparent motion of the meteor, compared with its height, some computation may be formed of its astonishing

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velocity.

velocity: As at the height of 50 miles above the surface of the earth, it might be visible from the same station for a tract of more than 1200 miles, and the longest continuance of its illumination scarcely exceeded a minute, we have hence some presumption that it moved not less than 20 miles in a second. The rev. Mr. WATSON, in his letter to Lord MULGRAVE, says, *that the arc described by it whilst in his view could not be less than 70° or 80° , and yet the time could not exceed $4''$ or $5''$ at most.* This, with an altitude of 60° , and height of 50 miles, gives for its velocity about 21 miles in a second. The observer at Newton Ardes estimated its motion to be 10° in a second, at the altitude of 16° ; this would make its velocity 30 miles in a second. Mr. HERSCHEL found it describe an arch of 167° during the 40 or 45 seconds he observed it, which gives a velocity of more than 20 miles in a second. Finally, Mr. AUBERT F. R. S. thought it described an arch of 136° of azimuth in 10 or 12 seconds, which would make its velocity above 40 miles in a second. I am sensible of the objections that may be made to all these computations; undoubtedly they are too vague; and yet, all taken together, perhaps they may have some weight, especially as they correspond so well with the different phænomena of the meteor's duration, and other fire-balls have been computed to move as fast*. Stating the velocity at the lowest computation of 20 miles a second, it exceeds that of sound above 90 times, and begins to approach toward that of the earth in her annual orbit. At such a rate, it must have passed over the whole island of Great Britain in less than half a minute, and might have reached Rome within a minute

* See Mem. de l'Acad. des Scienc. 1771, p. 678. Phil. Trans. N^o 341. and 360. and vol. LI. p. 263, &c.

afterwards, or in seven minutes have traversed the whole diameter of the earth !

From this calculation it will be evident, that there is little chance of determining the velocity of meteors from the times of their passing the zenith of different places ; and that therefore we must principally depend on observing carefully, with a watch that shews seconds, their apparent velocity through the heavens.

THE fire-ball which appeared on the 4th of October, at 43' past six in the evening, was much smaller than that already described, and of much shorter duration. It was first perceived to the northward as a stream of fire, like the common shooting stars, but large ; and having proceeded some way under this form, it suddenly burst out into that intensely bright bluish light which is peculiar to such meteors. At this period I saw it, and can compare the colour to nothing I am acquainted with so well, as to the blue lights of India, and some of the largest electrical sparks. The illumination was very great, and on that part of its course where it had been so bright, a dusky red streak or train was left, which remained visible perhaps a minute even with a candle in the room, and was thought by some gradually to change its form. Except this train, I think the meteor had no tail, but was nearly a round body, or perhaps a little elliptical. After moving not less than 10° in this bright state, it became suddenly extinct, without any appearance of bursting or explosion.

This meteor was seen for so short a way, that it is scarcely possible to determine the direction of its course with accuracy ; but as in proceeding to the eastward it very perceptibly inclined towards the horizon, it certainly moved somewhere from the north-westward to the south-eastward. Its duration was so

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short,

short, that many persons thought it passed in an opposite direction; for my own part, I found myself absolutely unable to determine whether the motion was *from* or *toward* the S.E. Some spectators were of opinion, that it changed its course the moment it became bright, proceeding no longer in the same straight line; but my information is not sufficient to determine this question.

My situation, Sir, was particularly fortunate for ascertaining the height of this meteor, as I saw it from your Library, ranging immediately over the opposite roof of your house. Hence I find by a quadrant that its altitude, even when it became extinct, could not be less than 32° . The upper northernmost end of the train it left bore, as I judge by the compass, about 28° northward of true E. and the lower end about 14° . I have only one observation to compare with this, which was made by Mr. Boys of Sandwich. He concludes, from the train I imagine, that "it disappeared just under, and a very "little to the westward" (rather northward) "of, the star " γ in the foot of Cepheus." At that time γ Cephei was about 57° high, and bore above 21° to the eastward of N. whence the height of the meteor above the surface of the earth, after all proper allowances are made*, must have been between 40 and 50 miles.

As there was no appearance of bursting at the extinction of this fire-ball, so no report was heard after it; nor did any sound attend it.

Some observers thought *this* meteor also near as big as the moon, but to me it did not appear above one quarter of her diameter, which would make its breadth somewhat above a furlong.

* It appears from observations taken by Gen. Roy, F. R. S. that the bearing of Sandwich from London is not so much to the southward of east, as it is laid down in our maps.

If

If the whole of the meteor's track be included, it seems to have lasted as much as three seconds, but in the bright state its duration was less than two, I think not much above one. Supposing it described an arc of 14° in $1\frac{1}{2}$ second, or, according to Mr. AUBERT's observation, of 25° in $3''$, its real velocity was about 12 miles a second.

Such meteors as these, which pass like a flash of lightning, and describe so short a course, are very unfavourable for calculating the velocity, but afford great advantages for determining the height, as they must be seen nearly at the same moment and in the same place by the different observers. Other instances are found of fire-balls beginning with a dull red light like a falling star, particularly the great one of March 19, 1719, treated of so fully by Dr. HALLEY * and Mr. WHISTON †.

It is remarkable, that a similar meteor had appeared the same day, that is, Saturday the 4th of October, about three in the morning, though, on account of the early hour, it was seen by fewer spectators. They represent it as rising from the northward to a small altitude, and then becoming stationary with a vibratory motion, and an illumination like day-light; it vanished in a few moments, leaving a train behind. This sort of tremulous appearance has been noticed in other meteors, as well as their continuing stationary for some time, either before they began to shoot forward, or after their course was ended.

* Phil. Transf. vol. XXX. N^o 350. p. 978.

† Account of a surprising meteor seen March 19, 1719.

I FIND it, Sir, impossible to quit this subject, without some reflexions about the cause, that can be capable of producing such appearances at an elevation above the earth, where, if the atmosphere cannot absolutely be said to have ceased, it is certainly to be considered as next to nothing. The first idea which suggested itself, that they were burning bodies projected with such a velocity, was quickly abandoned, from the want of any known power to raise them up to that great height, or, if there, to give them the required impetus; and the ingenuity of Dr. HALLEY soon furnished him with another hypothesis, in which he thought both these difficulties obviated. He supposes there is no projection of a single body in the case; but that a train of combustible vapours, accumulated in those lofty regions, is suddenly set on fire, whence all the phenomena are produced by the successive inflammation *. But Dr. HALLEY gives no just explanation of the nature of these vapours, nor of the manner in which they can be raised up through air so extremely rare; nor, supposing them so raised, does he account for their regular arrangement in a straight and equable line of such prodigious extent, or for their continuing to burn in such highly rarefied air. Indeed, it is very difficult to conceive, how vapours could be prevented, in those regions where there is in a manner no pressure, from spreading out on all sides in consequence of their natural elasticity, and instantly losing that degree of density which seems necessary for inflammation. Besides, it is to be expected, that such trains would sometimes take fire in the middle, and so present the phenomenon of two meteors at the same time, receding from one another in a direct line.

These difficulties have induced other philosophers to relinquish Dr. HALLEY's hypothesis, and propose, instead of it,

* Phil. Transf. vol. XXX. N^o 360.

one of a very opposite nature, that meteors are permanent solid bodies; not raised up from the earth, but revolving round it in very eccentric orbits; or, in other words, that they are terrestrial comets*. The objections to this opinion, however, seem to me equally great. Most observers describe the meteors, not as looking like solid bodies, but rather like a fine luminous matter, perpetually changing its shape and appearance. Of this many defenders of the opinion are so sensible, that they suppose the revolving body gets a coat or atmosphere of electricity, by means of which it becomes luminous; but, I think, whoever carefully peruses the various accounts of fire-balls, and especially ours of the 18th of August when it divided, will perceive that their phænomena do not correspond with the idea of a solid nucleus enveloped in a subtle fluid, any more than with the conjecture of another learned gentleman, that they become luminous by means of a contained fluid, which occasionally explodes through the thick solid outer shell †.

A strong objection to this hypothesis of permanent revolving bodies, is derived from the great number of them there must be to answer all the appearances. Such a regular gradation is observed, from those large meteors which strike all beholders with astonishment, and occur but rarely, down to the minute fires called shooting stars, which are seen without being regarded in great numbers every clear night, that it seems impossible to draw any line of distinction between them, or deny that they are all of the same nature. But such a crowd of revolving bodies could scarcely fail to announce their existence by some other means than merely a luminous train in the night;

* See a dissertation on this subject by Professor CLAP, of Yale College, New England.

† Phil. Trans. vol. LI. p. 267.

as, for instance, by meeting or justling sometimes near the earth, or by falling to the earth in consequence of various accidents; at least we might expect they would be seen in the day-time, either with the naked eye or telescopes, by some of the numerous observers who are constantly examining the heavens. With regard to these falling stars, it were much to be wished, that observations should be made upon them by different persons in concert at distant stations, for the purpose of ascertaining their height and velocity; which would tend very much to illustrate all this part of meteorology.

Another argument of great weight against the hypothesis that fire-balls are terrestrial comets, is taken from their great velocity. A body falling from infinite space toward the earth, would have acquired a velocity of no more than 7 miles a second, when it came within 50 miles of the earth's surface; whereas these meteors seem to move at least three times faster. And this objection, if there be no mistake in regard to the velocity of the meteors, as I think there is not, absolutely over-sets the whole hypothesis.

What then can these meteors be? The only agent in nature with which we are acquainted, that seems capable of producing such phenomena, is electricity. I do not mean that by what is already known of that fluid, all the difficulties relative to meteors can be solved, as the laws, by which its motions on a large scale are regulated in those regions so nearly empty of air, can scarcely, I imagine, be investigated in our small experiments with exhausted vessels*; but only that several of the facts point out a near connexion and analogy with electricity, and that none of them are irreconcilable to the discovered laws of that fluid.

* How nearly the phenomena of meteors have been represented by artificial electricity is known from a very remarkable experiment of Mr. ARDEN's. See PRIESTLEY, vol. V. p. 379.

1. Electricity moves with such a prodigious velocity, as to elude all the attempts hitherto made by philosophers to detect it; but the swiftness of meteors, stating it at 20 miles a second, is such as no experiments yet contrived could have discovered, and which seems to belong to electricity alone. This is, perhaps, the only case in which the course or direction of that fluid is rendered perceptible to our senses, in consequence of the large scale on which these fire-balls move.

2. Various electrical phenomena have been seen attending meteors. Lambent flames are described as settling upon men, horses, and other objects*; and sparks coming from them, or the whole meteor itself, it is said, have damaged ships, houses, &c. in the manner of lightning†. These facts, I must own, are but obscurely related, yet still they do not seem to be destitute of foundation. If there be really any hissing noise heard while meteors are passing, it seems explicable on no other supposition than that of streams of electric matter issuing from them, and reaching the earth with a velocity equal to that of the meteor, namely, in two or three seconds. Accordingly, in one of our late meteors, the hissing was compared to that of electricity issuing from a conductor‡. The sparks flying off so perpetually

* PRIESTLEY's History of Electricity, p. 352. Mem. de l'Acad. des Scienc. 1771, p. 681, 682. See an odd fact, perhaps of this nature, in PARKER's General Advertiser, Dec. 1, 1783.

† Mem. anc. de l'Acad. de Dijon, tom. I. Hist. p. 42. Phil. Transf. vol. XLVI. p. 366. Hist. de l'Acad. des Scienc. 1761, p. 28.

‡ Chester Weekly Courant, August 26, 1783. This and many other curious circumstances, relative to meteors, are so well exemplified in the following observation, made several years ago by Mr. ROBINSON at Hinckley in Leicestershire, that I think it worth transcribing here, especially as it occurs in a work which few people would think of consulting on such a subject. "Oct. 26, 1766, at half past five in the evening, after a violent storm of wind and rain,
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perpetually from the body of fire-balls, may possibly have some connexion with these streams *. In the same manner the sound of explosions may perhaps be brought to us quicker, than if it were propagated through the whole distance by air alone. Should these ideas be well founded, the change of direction which meteors seem at times to undergo, may possibly be influenced by the state of the surface of the earth over which

" I observed a fiery meteor. Its direction was from N.W. to S.E. nearly in a horizontal direction; it passed very near to me, and was of an elliptical form; its motion about 40° in 2" or 3" of time. It was very bright and lucid to appearance like the palest lightning, and emitted sparks continually, which formed a kind of tail toward the N.W. which seemed to be extinguished at the distance of 2° or 3° from the body; there was a small portion that parted from it. The cohesion of matter was so great, that it drew a thread of considerable length from the body, before it broke from it. During the passage there was a kind of *hissing noise, much like to what we hear from the electrical machine when the electric matter is running away, or as when it is escaping from a full charged jar.*" Bibliotheca Topographica Britannica, N^o VII. p. 81.

* Hist. de l'Acad. des Scienc. 1761, p. 28. Mem. de l'Acad. des Scienc. 1771, p. 682. Extract of a letter from the Abbé MANN, Director of the Academy at Brussels, to Sir JOSEPH BANKS, Bart. P. R. S. " I shall only mention one singular circumstance, which was communicated to me by a particular friend of mine. It happened at Mariekercke, a small village on the coast, about half a league to the W. of Ostend. The curate of the village was sitting in the dusk of the evening with a friend, when a sudden light surprised them, and immediately after a small ball of light-coloured flame came through a broken pane of glass, crossed the room where they were sitting, and fixed itself on the chink of a door opposite to the window where it entered, and there died gradually away. It appeared to be a kind of phosphoric light, carried along by the current of air. The curate and his friend, greatly surprised at what they saw, apprehended fire in the neighbourhood; but going out, found that the fire, which had come in through the window, had been detached from a large meteor in its passage."

How far these and similar appearances may be owing simply to the illumination produced by meteors, should be attentively considered in the investigation of such facts.

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they are passing, and to which the streams are supposed to reach. A similar cause may occasion the apparent explosion, the opening of more channels giving new vent and motion to the electric fluid. May not the deviation and explosion which appear to have taken place in the fire-ball of the 18th of August over Lincolnshire, have been determined by its approach toward the fens, and an attraction produced by that large body of moisture?

3. A further argument for the electric origin of meteors is deduced from their connexion with the northern lights, and the resemblance they bear to these electrical phænomena, as they are now almost universally allowed to be, in several particulars. Instances are recorded, where northern lights have been seen to join and form luminous balls, darting about with great velocity, and even leaving a train behind like the common fire-balls*. This train I take to be nothing but the rare air left in such a highly electrified state as to be luminous; and some streams of the northern lights are very much like it. The *aurora borealis* appears to occupy as high, if not a higher, region above the surface of the earth, as may be judged from the very distant countries to which it has been visible at the same time†; indeed the great accumulation of electric matter seems to lie beyond the verge of our atmosphere, as estimated by the cessation of twilight. Also with the northern

* Hist. de l'Acad. des Scienc. 1705, p. 35. WHISTON'S Account of a Meteor seen in the Air 171½. Phil. Transf. vol. XLI. p. 626; and LIII. p. 6? Also a most pointed fact in the Act. Liter. Sueciz, 1734, p. 78.

† BERGMAN, upon a mean of 30 computations, makes the average height of the northern lights to be near 70 Swedish, that is, about 460 English miles. Kong. Vetensk. Acad. Handlingar, vol. XXV. p. 193. See also Phil. Transf. vol. LIV. p. 327. and M. DE MAIRAN's Traité de l'Aurore Boreale, p. 51.

lights a hissing noise is said to be heard in some very cold climates; GMELIN speaks of it in the most pointed terms, as frequent and very loud in the north-eastern parts of Siberia *; and other travellers have related similar facts †.

But,

* *Reise durch Siberien*, vol. III. p. 135. As the whole passage is very remarkable, and has never, that I know, appeared in English, I thought the following translation of it might be acceptable.

“ These northern lights begin with single bright pillars, rising in the N. and almost at the same time in the N.E. which gradually increasing comprehend a large space of the heavens, rush about from place to place with incredible velocity, and finally almost cover the whole sky up to the zenith. The streams are then seen meeting together in the zenith, and produce an appearance as if a vast tent was expanded in the heavens, glittering with gold, rubies, and sapphire. A more beautiful spectacle cannot be painted; but whoever should see such a northern light for the first time, could not behold it without terror. For however fine the illumination may be, it is attended, as I have learned from the relation of many persons, with such a hissing, cracking, and rushing noise throughout the air, as if the largest fire-works were playing off. To describe what they then hear, they make use of the expression, *Spolochi chodjat*, that is, the raging host is passing. The hunters who pursue the white and blue foxes in the confines of the Icy Sea, are often overtaken in their course by these northern lights. Their dogs are then so much frightened, that they will not move, but lie obstinately on the ground till the noise has passed. Commonly clear and calm weather follows this kind of northern lights. I have heard this account, not from one person only, but confirmed by the uniform testimony of many, who have spent part of several years in these very northern regions, and inhabited different countries from the Yenisei to the Lena; so that no doubt of its truth can remain. This seems indeed to be the real birth-place of the *aurora borealis*.”

It is here to be observed, that GMELIN did not collect the account himself, but extracted it from letters or papers of M. DE L'ISLE DE LA CROYERE's, who was himself far to the northward of Yakutsk, without hearing these noises; probably, therefore, it is much exaggerated, though one can scarcely suppose the whole to be fabulous.

† *Musschenbroeck Introd. § 2495. Beccaria dell' Elettricismo artif. et nat.*

But, in my opinion, the most remarkable analogy of all, and that which tends most to elucidate the origin of these meteors, is the direction of their course, which seems, in the very large ones at least, to be constantly from or toward the north or north-west quarter of the heavens, and indeed to approach very nearly to the present magnetical meridian. This is particularly observable in those meteors of late years whose tracks have been ascertained with most exactness; as that of November 26, 1758, described by Sir JOHN PRINGLE; that of July 17, 1771, treated of by M. LE ROY; and this of the 18th of last August. The largest proportion of the other accounts of meteors confirm the same observation, even those of a more early period;*; nay, I think, some traces of it are per-

p. 221. There is now working with Mr. NAIRNE F. R. S. a person of the name of ARNOLD, who resided seven years at Hudson's Bay, the last three at Fort Henley. He confirms M. GMELIN's account of the fine appearance and brilliant colours of the northern lights, and particularly of their rushing noise, which he affirms he has very frequently heard, and compares it to the sound produced by whirling round a stick swiftly at the end of a string. He adds, that on conversing about this matter with a Swedish watch-maker of the name of LIND, that person assured him, that he had heard a similar noise in his own country. Mr. NAIRNE too, one time, at Northampton, when the northern lights were remarkably bright, is confident he perceived a hissing or whizzing sound.

This hissing or rushing noise, as well as that attending meteors in their passage, supposing it in both cases to be real, I would attribute to small streams of electric matter, running off to the earth from the great masses or accumulations of electricity, by which I suppose both meteors and the northern lights to be produced. Compare M. DE MAIRAN's *Traité de l'Aurore Boréale*, p. 126.

* See Phil. Trans. and Mem. de l'Acad. des Sciences, &c. I have found, of an earlier or later period, accounts of more than 40 different fire-balls. Of these above 20 are so described, that it is certain their course was in the above-mentioned direction; only 3 or 4 seem to have moved the contrary way; and with regard to the remainder, it is left doubtful, from the imperfect state of the relations.

perceivable in the writings of the ancients*. Whether their motion shall be *from* the northern quarter of the heavens or *toward* it, seems nearly indifferent, as the numbers of those going each way are not very unequal; I consider them, in the former case, as masses of the electric fluid repelled, or bursting from the great collected body of it in the north; and, in the latter case, as masses attracted toward that accumulation; a distinction, probably, much the same in effect, as that of positive and negative electricity near the surface of the earth.

This tendency toward the magnetic meridian, however, seems to hold good only with regard to the largest sort of fireballs; the smaller ones move more irregularly, perhaps because they come further within the verge of our atmosphere, and are thereby more exposed to the action of extraneous causes. That the smaller sort of meteors, such as shooting stars, are really lower down in the atmosphere, is rendered very probable by their swifter *apparent* motion; perhaps it is this very circumstance which occasions them to be smaller, the electric

tions. When we consider that even the meteor of the 18th of August last was thought by *some* spectators to move south-westward, it will rather appear surprising that so many of these accounts should correspond, than that a few of them should differ.

* ARISTOTLE (Meteor. lib. I. c. 6.) denies that comets, with which I take meteors to be confounded, are generated *only* in the north; which shows it to have been then the prevalent opinion, that they appeared most frequently in that quarter. Αλλα μη ουδὲ τὴν ἀλφειαν, ὅτι ἐν τῇ πρὸς ἀρκτον τοπῇ γινέται ὁ Κομήτης μόνον. So likewise PLINY (lib. II. c. 25.) Xiphias, Disceus, Pitheus doliorum cernuntur figura, in concavo fumidæ lucis. Ceratias. Lampadias. Hippeus. Candidus Cometes. Omnes ferme *sub ipso septentrione*, aliquâ ejus parte non certâ, sed maxime in candidâ, quæ lactei circuli nomen accepit. And SENECA (Quæst. Nat. lib. VII.) Placet ergo nostris, Cometas, sicut Tubas, Trabesque, et alia ostenta cæli, denso aëre creari. Ideo *circa septentrionem frequentissimè apparent*, quia illis plurimum est aëris pigri.

fluid

fluid being more divided in more resisting air. But as those masses of electricity, which move where there is scarcely any resistance, so generally affect the direction of the magnetic meridian, the ideas which have been entertained of some analogy between these two obscure powers of nature, seem not altogether without foundation *.

If the foregoing conjectures be just, distinct regions are allotted to the electrical phenomena of our atmosphere. Here below we have thunder and lightning, from the unequal distribution of the electric fluid among the clouds; in the loftier regions, whither the clouds never reach, we have the various gradations of falling stars; till beyond the limits of our crepuscular atmosphere the fluid is put into motion in sufficient

* It appears to me more rational to resolve this analogy into a power of electricity to influence magnetism, than into a supposed similarity of two fluids; as the former can be made evident by our artificial experiments, but there is no proof of the latter. When fire-balls, therefore, are said to affect the magnetic meridian, I do not mean that they are drawn in that direction because it is the line of magnetism, but rather that the magnetic poles of the earth are thrown into their present position, by the accumulation and action of that very electricity upon which the fire-balls depend. Should a change be produced by any cause in the place of this accumulation, or the state of its motion, it is not improbable, that the main polarity would be given to other portions of the earth, whence a variation in the pointing of the compass would necessarily ensue. If Dr. FRANKLIN'S hypothesis be admitted, ascribing the electrical state of the polar atmosphere to the crust of ice (a bad conductor) in those regions, it follows, that should ice form or be collected in one part more than in another, the atmosphere there would become more highly electrical, and, in so far as the magnetism is given by electricity, the adjoining portion of the earth would acquire a stronger polarity. Now it is certainly worthy of remark, that since our first northern navigations, the coast of West Greenland and its surrounding seas have become gradually more and more inaccessible on account of ice, and that the magnetic needle all this time has been constantly changing its variation to the westward.

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masses to hold a determined course, and exhibit the different appearances of what we call fire-balls; and probably at a still greater elevation above the earth, the electricity accumulates in a lighter less condensed form, to produce the wonderfully diversified streams and coruscations of the *aurora borealis*.

I have the honour to be, with the greatest respect,

S I R,

Your most obedient humble servant,

C. BLAGDEN.

END OF PART I. OF VOL. LXXIV.

PHILOSOPHICAL
TRANSACTIONS.

PART II.

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PHILOSOPHICAL
TRANSACTIONS,
OF THE
ROYAL SOCIETY
OF
L O N D O N.

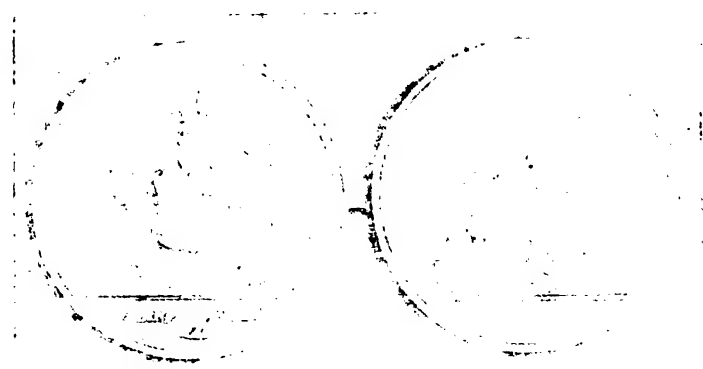
V O L. LXXIV. For the Year 1784.

P A R T II.

L O N D O N,

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PHILOSOPHICAL
TRANSACTIONS.

XIX. *On the remarkable Appearances at the Polar Regions of the Planet Mars, the Inclination of its Axis, the Position of its Poles, and its spheroidal Figure; with a few Hints relating to its real Diameter and Atmosphere.* By William Herschel, Esq. F. R. S.

Read March 11, 1784.

WHAT I have to offer on the subject of the remarkable appearances at the polar regions of Mars, as well as what relates to the inclination of the axis, the position of the poles, and the spheroidal figure of that planet, is founded on a series of observations which I shall deliver in this paper; and

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after

after they have been given in the order they were made, it will be easy to shew, by a few deductions from them, that my theory of this planet is supported by facts which will sufficiently authorise the conclusions I have drawn from them. For the sake of better order and perspicuity, however, I shall treat each subject apart, and begin with the remarkable appearances about the polar regions. The observations on them were made with a view to the situation and inclination of the axis of Mars; for to determine these we cannot conveniently use the spots on its surface, in the manner which is practised on the sun. The quantities to be measured are so small, and the observations of the center of Mars so precarious, and attended with such difficulties (since an error of only a few seconds would be fatal) that we must have recourse to other methods.

When I found that the poles of Mars were distinguished with remarkable luminous spots *, it occurred to me, that we might obtain a good theory for settling the inclination and nodes of that planet's axis, by measures taken of the situation of those spots. But, not to proceed upon grounds that wanted confirmation, it became necessary to determine by observation, how far these polar spots might be depended upon as permanent; and in what latitude of the globe of Mars they were situated; for, if they should either be changeable, or not be at the very poles, we might be led into great mistakes by overlooking these circumstances. The following observations will assist us in the investigation of these preliminary points.

* A bright spot near the southern pole, appearing like a polar zone, has also been observed by M. MARALDI. See Dr. SMITH's Optics, § 1094.

1777, April 17. 7 h. 50'. There are two remarkable bright spots on Mars. In fig. 1. tab. VI. they are marked *a* and *b*. The line AB expresses the direction of a parallel of declination. 10 feet reflector, 9 inches aperture, power 211*.

10 h. 20'. They are both quite gone out of the disk.

1779, This year, in all my observations on Mars, there is no mention of any bright spots, so that I believe there were none remarkable enough to attract my attention. However, as my view was particularly directed to the phenomena of this planet's diurnal rotation, it is possible I might overlook them.

1781, March 13. 17 h. 40'. 20 feet reflector. I saw a very lucid spot on the southern limb of Mars of a considerable extent. See fig. 2.

June 25. 11 h. 36'. 7 feet reflector, power 227. Two luminous spots appeared at *a* and *b*, fig. 3.; *a* is larger than *b*.

12 h. 15'. With 460. *a* is thicker than *b*, but *b* is rather longer.

13 h. 12'. *a* is grown thicker, and *b* become thinner.

June 27. 11 h. 20'. The two lucid spots are on Mars.

June 28. 11 h. 15'. They are both visible; *a*, fig. 4. is much thicker than *b*.

12 h. 55'. A line joining *a* and *b* does not go through the center.

June 30. 10 h. 48'. The spot *a* is visible. fig. 5.

11 h. 35'. Both spots are to be seen.

* Phil. Transf. vol. LXXI. p. 127. and fig. 17.

1781, July 3. 10 h. 54'. *a* seems to be larger than I have seen it, fig. 6.

11 h. 24'. *b* is not yet visible, fig. 7.

12 h. 36'. I perceive part of *b*, fig. 8.

July 4. 12 h. 9'. *a* is very full; *b* extremely thin, and barely visible.

12 h. 18'. *a* and *b* are not quite opposite each other.

12 h. 49'. *b* is increased.

July 15. 9 h. 54'. *a* is visible, fig. 9.

11 h. 35'. *b* invisible.

12 h. 12'. *b* not to be seen.

July 16. 11 h. 9'. The bright spot *a* is very large.

July 17. 11 h. 15'. No other bright spot but *a*.

July 19. 13 h. 31'. *a* visible.

July 20. 10 h. 3'. I suppose the bright spot *a* on Mars is, very nearly, the south pole; which therefore must lie in sight. There is no second bright spot *b* visible to night.

10 h. 56' *b* not visible; the night very fine.

July 22. 11 h. 14'. At *a* and *b*, fig. 10. are bright spots; *a* is larger than *b*. Most probably the south pole is in view, and the north pole just hid from our sight. If the spots are polar, or nearly so, then *a* must, on a supposition of the south pole's being in view, appear larger than *b*; and if *b* extend a little more from the north pole one way than another, it must be subject to some change in its appearance from the revolution of Mars on its axis.

July 30. 9 h. 43'. Both spots visible.

August 8. 10 h. 4'. Only *a* visible, fig. 11.

August 17. 9 h. 21'. Only *a* in sight.

1781, August 23. 8 h. 44'. *a* as usual, and part of *b* visible,
fig. 12.

Sept. 7. The white spot *a* is very large.

1783, May 20. Mars has a singular appearance. At *a*, fig. 13.
is the polar spot, which is bright, and seems to project
above the disk by its splendour, causing a break
at *c*.

July 4. *a* is very bright.

July 23. 14 h. 45'. *a* is very lucid.

August 16. I saw the bright spot with the 20 feet
reflector as usual.

Aug. 26. The lucid spot on Mars is its south pole, for
it remains in the same place, while the dark equato-
rial spots perform their constant gyrations: it is
nearly circular.

Aug. 29. The south polar spot is in the same situation.

Sept. 9. As usual.

Sept. 22. The south polar spot is of a circular shape,
and very brilliant and white. I had a beautiful and
distinct view of it when it was about the meridian,
and measured its little diameter in the equatorial di-
rection of Mars. With a power of 932 it gave
1" 41", and I saw it very distinctly. The outward
edge of the spot came just up to the upper limb; a
favourable haziness, taking off every troublesome
ray, gave me objects in general exceedingly well de-
fined, especially Mars.

Sept 23. 9 h. 55. The polar spot *a*, fig. 14. as usual.

Sept. 24. The same.

1783, Sept. 25. 12 h. 30'. The bright south polar spot *a*, fig.

15. seems to be fixed in its place, and goes nearly up to the margin of the disk; it is perfectly round.

12 h. 55'. The track of the equatorial spots is incurvated, being convex towards the north, see *c*, *g*, fig. 23.: this confirms the white spot's being at the south pole. With long attention I can perceive the edge of the disk of Mars beyond the spot, extending about $\frac{1}{4}$ diameter of the spot.

Sept. 26. 12 h. 10'. The spot *a* is in a line with the center and the end of the hook, fig. 16.

Sept. 27, 28, 29. The spot as usual.

Sept. 30. 10 h. 30'. The polar spot as in fig. 17.

Oct. 1. 9 h. 55'. I am inclined to think, that the white spot has some little revolution, and therefore is not with its center exactly at the pole of Mars; it is rather probable, that the real pole, though within the spot, may lie near the circumference of it, or one-third of its diameter from one of the sides. A few days more will shew it, as I shall now fix my particular attention upon it.

10 h. 17'. The bright spot is certainly not so far upon the disk as it used to be formerly, and is either reduced or has a small motion; which of the two is the case will be seen in a few hours.

13 h. 3'. The bright spot has a little motion; for it is now come farther into the disk.

I concluded now, in general, that none of the bright spots on Mars were exactly at the poles, though they could certainly not be far from them: for what has been just related of the

1st, 2d, and 3d of October 1783, shews plainly, that the appearance of the southern spot *a* was a little affected by the diurnal motion of the planet; and the observations of the 3d and 4th of July 1781, shew also that the spot *b* could not be exactly at the north pole; and that, perhaps, the visible branch of the latter extended pretty far towards the equator. However, the south polar spot of the year 1783, being very small and nearly round, afforded a good opportunity for determining its polar distance, by noting the different angles of position it assumed while Mars revolved on its axis; to this end many observations were taken at different hours of the same night, which will be found among the measures of the angles of position in the next division of my subject. And since the different degrees of brilliancy, as well as the proportional apparent magnitude of the spot, would also contribute to the investigation of this point, I continued my remarks on those particulars, as follows.

1783, Oct. 2. 7 h. 59'. The bright spot near the south pole is about half visible.

Oct. 4. 8 h. 0'. The polar spot seems to project above the disk as formerly, and is very small.

Oct. 5. 11 h. 13'. The spot is very small, and seems actually to be in the circumference.

11 h. 30'. The spot is small, and seems to be with its farthest side in the circumference of the disk; or it may, perhaps, be partly beyond it, and therefore not all in sight.

11 h. 50'. I see the spot much clearer than I did before.

13 h. 15'. The white spot is more in sight, and of its usual size, but does not seem much to change its position;

sition; however, what change there is shews that it has been beyond the pole, as it appears to have been direct while the equatorial spots were retrograde.

1783, Oct. 9. 11 h. 48'. The white polar spot increases in size. At 10 h. 35'. it was as in fig. 18. but is now larger, and coming round towards that part of its orbit which is nearest to us. See fig. 24.

Oct. 10. 6 h. 20'. I see no white polar spot; but the planet is too low for any observation to be depended on.

6 h. 55'. The white spot begins to be visible; at least I see it now, the planet being higher than before, fig. 19.

9 h. 55'. With 460, the white spot is considerably increased, and shews a circular form, fig. 20.

Oct. 11. 7 h. 46'. The bright spot is very visible; the evening fine; with 278.

Oct. 16. 7 h. 7'. The spot is very luminous.

9 h. 55'. It seems rather lengthened; perhaps it may be arrived at the extreme of its parallel of declination.

Oct. 17. 7 h. 47'. The white spot *a*, fig. 21. is very bright.

13 h. 7'. It is less in appearance than it was in the beginning of the evening.

Oct. 23. 6 h. 46'. The bright spot is very large and luminous; I suppose it to be in the nearer parts of its little orbit.

7 h. 11'. It is situated as in fig. 22.

Oct. 24. 7 h. 1'. The white spot is very large.

Oct. 27. 8 h. 45'. It is very large and round.

Nov. 1. 7 h. 47'. The spot is round and bright.

1783, Nov. 11. The deficiency of light which occasions Mars to appear gibbous, reaches over the south polar spot towards the preceding limb, and hides it.

Nov. 14. Mars is gibbous, and the polar spot is thereby rendered invifible.

Nov. 17. 6 h. 0'. The south polar spot is under the fal-cated defect of light.

6 h. 30'. I do not know whether there be not a faint glimpfe of the polar spot left; the weather is too bad to determine it.

I have added fig. 25. (tab. X.) to fhew the connection of the 15th, 17th, 18th, 19th, 20th, 21ft, and 22d figures, which complete the whole equatorial circle of appearances on Mars, as they were observed in immediate fucceffion. The center of the circle marked 17 is placed on the circumference of the inner circle, by making its diftance from the center of the circle, marked 15, answer to the interval of time between the two obfervations, properly calculated and reduced to fidercal meafure. The fame has been done with regard to the circles marked 18, 19, &c. And it will be found, by placing any one of thefe connected circles, fo as to have its contents in a fimilar fituation with the figures in the fingle representation which bears the fame number, that there is a fufficient refem-blance between them; but fome allowance muft undoubtedly be made for the unavoidable diftortions occafioned by this kind of projection.

In order to bring thefe obfervations on the bright fots into one view, I have placed them at the circumference of three circles (fee fig. 26, 27, 28. tab. VII. VIII. IX.) divided into de-grees, representing the parallels of declination in which they

revolved about the poles of Mars. The division of the circles marked 360 is where a spot passes that meridian of the planet which is turned towards the earth, and where, consequently, it appears to us in its greatest lustre. The motion of the spot is according to the numbers 30, 60, 90, and so on to 360. In calculating the daily places of the spots I have used the sidereal period of 24 h. 39' 21'', 67 determined in my paper on the rotation of Mars*; and have also made proper allowances for the alterations of the geocentric longitudes calculated from the situations of that planet given in the Nautical Almanack; by which means the sidereal is reduced to a proper synodical period.

The following three tables contain the result of the calculations, and serve to explain the arrangement of the observations in the circles. In the first column are the times when the observations were made. In the second, the sidereal places of the spot in degrees and minutes. In the third column are the geocentric longitudes of Mars at the time of the observations. In the fourth, the necessary corrections on account of these different longitudes. In the fifth column are the corrected or synodical places of the spots; and, according to the numbers in that column, they are marked on the circles, where consequently each spot is represented as it must have appeared to be situated at the time of observation.

* Phil. Transf. vol. LXXI. p 134.

TABLE

T A B L E I.

Time of observation.			Sider. place.		Geoc. longit.			Correction.		Synod. place.		
D.	H.	M.	D.	M.	S.	D.	M.	D.	M.	D.	M.	
June	25	11	36	350	31	9	24	35	+0	0	350	31
	25	12	15	0	0	9	24	35	+0	0	0	0
	25	13	12	13	52	9	24	34	+0	1	13	51
	27	11	20	357	28	9	24	12	+0	23	357	5
	28	11	15	316	40	9	24	1	+0	34	316	6
	30	10	48	290	56	9	22	38	+0	57	289	59
	30	11	35	302	23	9	23	38	+0	57	301	26
July	3	10	54	263	40	9	22	55	+1	40	262	0
	3	11	24	270	58	9	22	55	+1	40	269	18
	3	12	10	282	9	9	22	55	+1	40	280	29
	3	12	36	288	29	9	22	55	+1	41	286	48
	4	12	9	272	20	9	22	40	+1	55	270	25
	4	12	49	282	4	9	22	40	+1	55	280	9
	15	9	54	134	7	9	19	43	+4	52	129	15
	15	11	35	158	42	9	19	42	+4	53	153	49
	15	12	12	167	42	9	19	42	+4	53	162	49
	16	11	9	144	48	9	19	26	-5	9	137	39
	17	11	15	134	40	9	19	9	-5	26	129	14
	19	13	31	148	37	9	18	34	-6	1	142	36
	20	10	3	88	25	9	18	21	-6	14	82	11
20	10	56	101	19	9	18	20	-6	15	95	4	
22	11	14	86	32	9	17	49	-6	46	79	46	
30	9	43	347	46	9	16	5	-8	30	339	16	

T A B L E II.

Time of observation.			Sider. place.		Geoc. longit.			Correction.		Synod. place.		
	D.	H.	M.	D.	M.	S.	D.	M.	D.	M.	D.	M.
June	25	11	36	86	51	9	24	35	+1	40	88	31
	25	12	15	96	20	9	24	35	+1	40	98	0
	25	13	12	110	12	9	24	34	+1	39	111	51
	28	11	15	53	0	9	24	1	+1	6	54	6
	30	10	48	27	16	9	23	38	+0	43	27	59
	30	11	35	38	43	9	23	38	+0	43	39	26
July	3	10	54	0	0	9	22	55	+0	0	0	0
	4	12	9	8	40	9	22	40	-0	15	8	25
	15	9	54	230	27	9	19	43	-3	12	227	15
	15	10	12	234	50	9	19	43	-3	12	231	38
	15	11	35	255	2	9	19	42	-3	13	251	49
	15	12	12	264	2	9	19	42	-3	13	260	49
	16	11	9	339	8	9	19	26	-3	29	235	39
	19	13	31	244	57	9	18	34	-4	21	240	36
	20	10	3	184	45	9	18	21	-4	34	180	11
	20	10	56	197	39	9	18	20	-4	35	193	4
	30	9	43	84	6	9	16	5	-6	50	77	16

T A B L E III.

Time of observation.			Sider. place.		Geoc. longit.			Correñion.		Synod. place.	
D.	H.	M.	D.	M.	S.	D.	M.	D.	M.	D.	M.
Sept. 25	13	30	6	32	0	9	54	+6	44	13	16
Oct. 1	10	17	262	5	0	8	6	+4	56	267	1
1	13	3	302	29	0	8	5	+4	55	307	24
2	7	59	218	55	0	7	50	+4	40	223	25
4	8	0	200	0	0	7	15	+4	5	204	5
4	8	46	211	12	0	7	15	+4	5	215	17
5	11	13	237	23	0	6	55	+3	45	241	'8
5	11	30	241	31	0	6	55	+3	45	245	16
5	11	50	246	23	0	6	55	+3	45	250	8
5	13	15	267	4	0	6	54	+3	44	270	48
5	14	0	278	1	0	6	53	+3	43	281	44
7	8	20	176	8	0	6	23	+3	13	179	21
7	10	5	201	41	0	6	22	+3	12	204	53
7	11	50	227	14	0	6	21	+3	11	230	25
9	11	48	207	35	0	5	49	+2	39	210	14
10	6	55	126	42	0	5	37	+2	27	129	9
10	7	50	140	5	0	5	36	+2	26	142	31
10	9	55	170	30	0	5	34	+2	24	172	54
10	12	11	203	36	0	5	33	+2	21	205	57
16	7	7	72	9	0	4	15	+1	5	73	14
16	7	46	81	39	0	4	15	+1	4	82	43
16	9	55	113	2	0	4	14	+1	4	114	6
17	7	47	72	19	0	4	3	+0	53	73	12
17	13	7	150	11	0	4	0	+0	50	151	1
23	6	46	0	0	0	3	10	-0	0	0	0
24	7	1	354	0	0	3	2	-0	8	353	52

From the appearance and disappearance of the bright north polar spot in the year 1781, we collect that the circle of its motion, represented by fig. 26. was at some considerable distance from the pole. By a calculation, made according to the principles hereafter explained, its latitude must have been about 76° or 77° north; for I find that, to the inhabitants of Mars, the declination of the sun, June 25. 12 h. 15' of our time, was about 9° 56' south*; and the spot must have been at least so

* See p. 259. and 260.

far

far removed from the north pole as to fall a few degrees within the enlightened part of the disk, to become visible to us.

The south pole of Mars could not be many degrees from the center of the large bright southern spot of the year 1781, whose course is traced in fig. 27; though the spot was of such a magnitude as to cover all the polar regions farther than the 70th or 65th degree, and in that part which was on the meridian July 3, at 10 h. 54, perhaps a little farther.

In the next division of our subject will be shewn, that the inclination and position of the axis of Mars are such, that the whole circle, fig. 23. (which will appear to be in about $81^{\circ} 52'$ of south latitude on the globe of Mars) was in view all the time the observations on the bright south polar spot of the year 1783, which are marked upon it, were made, but in so oblique a situation as to be projected into a very narrow ellipsis. See fig. 24. where *m n* is the little ellipsis in which the spot *a* revolved about the pole. Hence then we may easily account for the observed magnitude and brightness of the spot Oct. 23, 24, and 27. when it was exposed to us in its meridian splendour. Its situations Oct. 16. and 17. on one extreme of the parallel, as well as those of Oct. 5. and Nov. 1. on the other, gave us also a bright view of it: and, when we pass over to that half of the circle which lies beyond the pole, the much greater obliquity into which the spot must there be projected will perfectly account for its being smaller at 13 h. 7' of Oct. 17. than at 7 h. 47' of the same evening. It will also explain its smallness Oct. 4. and its increase Oct. 9. We shall have occasion hereafter to recur to the same figure, so that I take no notice at present of the angles of position which are marked upon it.

Of the direction or nodes of the axis of Mars, its inclination to the ecliptic, and the angle of that planet's equator with its own orbit.

From the foregoing article we may gather, that the bright polar spots on Mars are the most convenient objects for determining the situation of the axis of this planet; I shall therefore collect, in one view, all the measures I have taken of these spots for that purpose. Before I constructed a micrometer for taking the angle of position, I used to draw a line through the figure delineated of Mars to represent the parallel of declination; in a few of my first observations, therefore, I can only take the situation of the polar spots from such drawings, and of consequence no great accuracy in the angles, as to the exact number of degrees, can be expected.

1777, April 17. 7 h. 50'. A line drawn through the middle of the two bright polar spots *a* and *b*, fig. 1. makes an angle of about 63° , with a parallel of declination AB; the southern spot preceding and the northern following.

My reason for chusing a line drawn through both the spots rather than through one of them and the center is, first, that they were not situated quite opposite each other, and therefore, unless other observations had pointed out which was most polar, I should evidently run the greater risk in fixing on one of them in preference to the other. In the next place, we find by the second observation, page 235. that in two hours and a half both spots were intirely gone out of the disk. This

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plainly.

plainly denotes, that they were both in the same half of a sphere orthographically projected, and divided by a plane passing through the axis of Mars and the eye, but that neither of them were polar. Now, a line drawn through two points not far from opposite each other, both in the same hemisphere, and both removed from the poles of it, must approach more to a parallelism with the axis, than a line drawn through either of them and the center.

1779, May 9. There being no bright spots by which to judge of the position of the poles, it is estimated from a well known dark equatorial spot, with a line drawn through the figure to denote a parallel of declination. By very rough estimation it is about 42° south preceding.

May 11. The same figure, being drawn again in another situation, and also with a line giving a parallel of declination, points out, by the same rough estimation, 62° south preceding.

1781, June 25. 11 h. 35'. The position of the spots *a* and *b*, fig. 3. with regard to a parallel of declination, measured with a micrometer $74^{\circ} 32'$. The spot *a* was south preceding, and *b* north following.

July 15. 10 h. 12'. The angle of position, of the center of the spot *a*, fig. 9. through the center of the disk, $74^{\circ} 18'$ south preceding.

1783, August 16. Position of the spot *a*, 64° south following the center; but as the planet is not full, the center becomes dubious, and the measure therefore may not be quite accurate, though taken with a 20 feet reflector; power 200.

Sept.

1783, Sept. 9. Position of the supposed south pole of Mars
 $65^{\circ} 12'$ south following; 7 feet reflector; power 460.

Sept. 22. Position of the same $52^{\circ} 9'$ f. following; 460.

Sept. 25. 13 h. 30'. Position of the south polar spot
 $56^{\circ} 27'$. very accurately taken, by bisecting the
 disk of Mars through the bright spot, and supposing
 the planet now near enough the opposition to induce
 no material error; Hitherto I have taken it through
 a supposed center by endeavouring to allow a little
 for what I thought the deficiency in the disk; but
 not to-night.

Oct. 4. 8 h. 46'. Position of the spot $51^{\circ} 20'$; Mars
 too low and hazy to depend much on the measure
 with so high a power as 460.

Oct 5. The motion of the polar spot being now strongly
 suspected, or rather already known, I took the fol-
 lowing measures, by way of discovering its quantity.
 Oct. 5. Position very exactly taken $50^{\circ} 6'$ f. fol-
 lowing.

Oct. 12. Position of the spot $49^{\circ} 45'$.

Oct. 14. 8 h. 20'. Position $55^{\circ} 12'$. In order to see how
 far this measure might be trusted to, I set $49^{\circ} 36'$ in
 the micrometer, which was evidently too small;
 next I took $51^{\circ} 36'$, which was also too small; after
 this, I took a new measure, and found $55^{\circ} 24'$,
 which appeared to me very exact. 10 h. 5'. The
 position now was 53° . 11 h. 50'. It measured $52^{\circ} 12'$.

As there is nothing to distinguish the center, it is ex-
 tremely difficult to please one's self in bringing the
 spot into a line with it.

1783, Oct. 10. 7 h. 30'. Position of the polar spot $57^{\circ} 12'$; with 460, very accurate. I tried a few parts less of the micrometer, but found the measure too little. I see pretty distinctly, but the air is tremulous;

9 h. 55'. Position $52^{\circ} 42'$; very distinct.

12 h. 11'. Position $46^{\circ} 36'$; I see not quite so well now as I could wish.

14 h. 1'. Position $44^{\circ} 12'$; but liable to great uncertainty, on account of tremulous air; it becomes more difficult to distinguish the center when the planet is not perfectly defined.

Oct. 16. 9 h. 57'. Position $65^{\circ} 9'$. By way of trial I set $59^{\circ} 36'$, which was too small; also $60^{\circ} 24'$ was too small; again, $61^{\circ} 24'$ was not large enough. Then, taking a fresh measure, I found it $62^{\circ} 48'$, which I thought right.

9 h. 33'. I took three measures, and thought the third, which was $63^{\circ} 6'$, the best of all, for I saw the planet and the spot remarkably well.

Oct. 27. 8 h. 45'. Position of the polar spot $59^{\circ} 30'$. I took three other measures, of which $56^{\circ} 39'$ appeared to me the best; it was taken with long attendance and many changes, and trials of the wires in different positions; but the gibbosity of Mars is such, that measures of the situation of the spot are now no longer to be depended on.

These positions, I believe, will be sufficient for the purpose of settling the latitude of the polar spots, and thereby obtaining a correct measure of the situation of the real pole. I have referred those of the south polar spot of the year 1783 to the same circle which contains the observations that were made on

the apparent brightness and magnitude of that spot, that they may be compared together. (See fig. 28.) The agreement of the measures, and the phænomena attending the motion of the spot, are sufficient to point out the meridian of the circle; for which, from a due consideration of these circumstances, I have fixed on the place where the spot was Oct. 10. 6 h. 46'.

Of the angles collected in fig. 28. we find $65^{\circ} 0'$ the largest, and $49^{\circ} 45'$ the smallest; but, on account of the different situation of the earth and Mars, the angle measured 7' less Oct. 16. than it would have done had the planets remained in the places they were in Oct. 5. when the other measure was taken. This being added, we have $65^{\circ} 7'$. The difference between the two positions is $15^{\circ} 22'$. Now, the construction of fig. 28. being admitted, we see that the angles were nearly taken at the opposite extremes of the circle in which the spot moved. However, by the 5th column of Tab. III. Oct. 5. we have the situation of the spot in the circle with respect to the meridian $281^{\circ} 44'$, and Oct. 16. $114^{\circ} 6'$: therefore the south polar distance of the center of the spot is found, by taking half the sum of the sines of these angles to radius, as $7^{\circ} 41'$ (half of $15^{\circ} 22'$) to a fourth number, which is $8^{\circ} 8'$; and the latitude of the circle, in which the spot moved about the pole, therefore is $81^{\circ} 52'$ south. This being determined, we have the following correction for the angles of position: radius is to sine of the angular distance of the spot from the meridian as $8^{\circ} 8'$ to the required quantity. This must be added or subtracted, according as the case requires; and thereby we shall have the position of the true pole from any one of the measures.

I shall now apply the above to determine the situation of the axis of Mars. To this end, we see that, in the first place, the

measures must be corrected for the latitude of the spot; next, they must be reduced to a heliocentric observation, which will also correct them from the difference occasioned by the different situation of the planets when they were taken. This being done, we may select two observations at a proper distance; from which, by trigonometry, we shall have the node and inclination of the axis. When these elements are obtained, it will be easy to see how other observations agree with them; which will afford the means of correcting or verifying the former calculations.

Let T , fig. 29. (tab. X) be the earth; $\odot Q q W$ the ecliptic as seen from T ; P the point of the heavens towards which the north pole of the earth is directed; M the place of the orbit of Mars $\mu m M$, where an observation of the poles of that planet has been made, which is to be reduced to its heliocentric measure. And, first, suppose it to have been made at the time of the opposition of that planet. Then, the place M or Q in the ecliptic being given, we have the sides $Q \odot$, $\odot P$; whence the angle Q of the right-angled triangle $P \odot Q$, is found. This being added to, or taken from, the observed angle of position of the axis of Mars, according to circumstances easily to be determined, reduces it to its heliocentric position. But if this observation was not made at the time of an opposition, but at some other place m , a second correction is to be applied in the following manner.

Let the angle q , of the triangle $P \odot q$, be found as before, and properly applied to the position of the axis of Mars now at m ; then make the angle $m S \mu$, at the sun S , equal to the angle $S m T$, and μ will be the heliocentric place, where the angle of position, when seen from S , will appear to be as it was found at m , after the application of the first correction:

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for $S\mu$ being parallel to Tm , and supposing the axis of Mars to preserve its parallelism while it moves from m to μ , appearances of Mars at μ to an eye at S , must be the same as they are at m to an eye at T .

The following table contains the result of calculations relating to the angles of Fig. 28. In the first column are the times when the observations were made. In the second, the angles as they were taken. In the third column are the quantities of the angles Q , q , calculated from the geocentric longitudes contained in the third column of the third table. In the fourth column are the corrections for the situation of the spot in the circle of latitude obtained from the sines of the angles in the fifth column of the third table. In the fifth are the corrections requisite on account of the change of situation of the planets, during the interval between the several days on which the measures were taken; these are obtained from the third column of this table, and I have assumed the 4th of October, as being the observation nearest the opposition, to which I have reduced the other measures. In the sixth column are the angles of the second, corrected by the quantities contained in the fourth and fifth columns, applied according to their signs.

TABLE

T A B L E IV.

Time of observation.			Angles taken.		Angle Q_2		First correction.		Second correct.	Angles corrected.	
D.	H.	M.	D.	M.	D.	M.	D.	M.	M.	D.	M.
Sept. 25	13	30	56	27	+23	10	-1	52	-8	54	27
Oct. 4	8	46	51	21	+23	18	+4	42	-0	56	3
	5	11	50	6	+23	19	+7	39	+1	57	46
	5	14	0	49	+23	19	+7	59	+1	57	45
	7	8	20	{ 55 12 }	+23	21	-0	7	+2	{ 55 7 }	
				{ 55 24 }						{ 55 19 }	
	7	10	5	53	+23	21	+3	26	+3	56	29
	7	11	50	52	+23	21	+6	16	+3	58	31
	10	7	50	57	+23	22	-4	57	+4	52	19
	10	9	55	52	+23	22	-1	7	+4	51	39
	16	7	7	{ 63 9 }	+23	25	-7	47	+7	{ 55 29 }	
				{ 62 48 }						{ 55 8 }	
	16	9	55	65	+23	25	-7	23	+7	57	45

As we have no particular reason to select one measure rather than another, a mean of all the 13 will probably be nearest the truth; so that by these observations, which, as we said before, are reduced to the 4th of October, 1783, we find the position of the axis of Mars that day to have been $55^{\circ} 41'$ south following.

From the appearances of the south polar spot in 1781, represented fig. 27. we may conclude, that its center was nearly polar. We find it continued visible all the time Mars revolved on its axis; and, to present us generally with a pretty equal share of the luminous appearance, a spot which covered from 45° to 60° of a great circle on the globe of Mars could not have any considerable polar distance: however, a small correction in the angle of position seems to be necessary, which should be taken from the measure of the 15th of July, because that branch of the spot which probably extended farthest towards the

the equator, was then in the *following* quadrant. The measure of both the spots on June the 25th, 1781, is still more to be depended on, as giving us very nearly the position of the true pole; for it appears evident from the phenomena of the bright north-polar spot in fig. 26. that that spot was in the meridian when the measure was taken, while the southern spot was in the *preceding* quadrant near its greatest limit. Now, since an angle at the circumference of a circle is but half the angle at the center, when the arches which subtend these angles are equal, the correction necessary to be applied to the measure taken through the two spots, will be but one half of the correction which would have been requisite had it been taken through the center; therefore, in order to reduce this to the condition of the former, we may suppose it to have been taken through the center of Mars when the spot was only 30, or 150 degrees from the meridian. It is also necessary to add $1^{\circ} 54'$ to the angle of July 15, which it would have measured more had the planets remained where they were June 25. This done, we may have the polar distance of the center of the spot as before. Half the sum of the sines (of $231^{\circ} 38'$ and 150°) to radius, as 50 (half the difference between $74^{\circ} 32'$ and $76^{\circ} 12'$) to a fourth number, which is $1^{\circ} 18'$.

I should observe here, that the measures of the angle of position would be too large before the spot came to the meridian, and too small afterwards, the axis of Mars being south preceding; whereas, in fig. 28. they would be too small before, and too large after, the meridian passage, the pole being south following.

These two observations arranged as those in the fourth table, and reduced to the time of the 25th of June, will stand as follows.

TABLE

T A B L E V.

Time of observation.	Angles taken.	Angle Q.	First correction.	Second correct.	Corrected Angle.
D. H. M.	D. M.	D. M.		D. M.	D. M.
June 25 11 36	74 32	- 10 14	+ { half of 1° 18'	- 0 0	75 11
July 15 10 12	74 18	- 8 20	- 1 1	+ 1 54	75 11

I am to remark, that we have here admitted both measures as equally good; and that, therefore, the result is a mean of them both, and shews the axis of Mars, June 25, 1781, to have been $75^{\circ} 11'$ south preceding.

Our next business will be to reduce these two geocentric observations to a heliocentric measure. This is to be done, as we have shewn before, by a calculation of the angle Q, fig. 29. The result of it shews, that $10^{\circ} 14'$ are to be subtracted from the mean corrected angle of position, reduced to June 25, 1781, and $23^{\circ} 18'$ to be added to the angle which is the corrected mean of 13 measures, reduced to Oct. 4, 1783. Hence we learn, that on those days and hours, when the heliocentric places of Mars were $9\text{ s. } 24^{\circ} 35'$, and $0\text{ s. } 7^{\circ} 15'$ (which would happen about July 18, 1781, and Sept. 29, 1783) an observer placed in the sun would have seen, on the former, the axis of Mars inclined to the ecliptic $64^{\circ} 57'$, the north pole being towards the left; and on the latter, he would have seen the same axis inclined to the ecliptic $78^{\circ} 59'$, the north pole being then towards the right.

The first conclusion we may draw from these principles is, that the north pole of Mars must be directed towards some point of the heavens between $9\text{ s. } 24^{\circ} 35'$ and $0\text{ s. } 7^{\circ} 15'$; because the change of the situation of the pole from left to right, which

which happened in the time the planet passed from one place to the other, is a plain indication of its having gone through the node of the axis. Next, we may also conclude, that the node must be considerably nearer the latter point of the ecliptic than the former; for, whatever be the inclination of the axis, it will be seen under equal angles at equal distances from the node.

But, by a trigonometrical process of solving a few triangles, we soon discover both the inclination of the axis, and the place where it intersects the ecliptic at rectangles (which, for want of a better term, I have perhaps improperly called its node). Accordingly I find, by calculation, that the node is in $17^{\circ} 47'$ of Pisces, the north pole of Mars being directed towards that part of the heavens; and that the inclination of the axis to the ecliptic is $59^{\circ} 42'$.

We shall now compare the observations of an earlier date with these principles, to see how far they agree. Some of the particulars and calculations relating to them are as follow.

T A B L E VI.

Times of Observation.			Estimations.	Geoc. longit.			Angle Q.	2d correct.
D.	H.	M.	D.	S.	D.	M.	D.	M.
1779, May 9	12	0	42	7	22	20	+ 14	45
May 11	12	0	62	7	21	40	+ 15	11
1777, Apr. 17	7	50	63	6	3	34	+ 23	26

May the 9th, 1779, as we have seen, the angle of position was roughly estimated at 42° , and May 11. at 62° . The great disagreement of these coarse estimations is undoubtedly owing to the very different situation of the dark spot from which they

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were

were taken ; however, since we do not mean to use these observations in our calculations, they may suffice in a general way to shew, that the axis of Mars was actually about that time in such a situation as our principles give it : for, reducing the two positions to the 9th of May, that of the 11th, from an allowance of $26'$ for the situation of the planets, will become $62^{\circ} 26'$; and a mean of the two, $50^{\circ} 13'$ south preceding ; which, reduced to a heliocentric observation, gives $66^{\circ} 30'$, the north pole lying towards the left. Now, on calculating from the position of the node and inclination of the axis before determined, we find, that the heliocentric angle was $62^{\circ} 49'$, the north pole pointing towards the left ; and a nearer agreement with these principles could hardly be expected from estimations so coarse. If we go to the year 1777, and take the position of the two bright spots observed the 17th of April, we have 63° south preceding ; this, reduced to a heliocentric quantity, gives $86^{\circ} 26'$ of inclination, the north pole being to the left. By calculating we find, that that pole was then actually $81^{\circ} 27'$ inclined to the ecliptic, and pointed towards the left as seen from the sun.

The inclination and situation of the node of the axis of Mars with respect to the ecliptic being found may thus be reduced to that planet's own orbit. Let EC, fig. 30. (tab. X.) be a part of the ecliptic ; OM part of the orbit of Mars ; PEO a line drawn from P, the celestial pole of Mars, through E, that point which has been determined to be the place of the node of the axis of Mars in the ecliptic, and continued to O where it intersects the orbit of Mars. Now, if according to Mr. DE LA LANDE we put the node of the orbit of Mars for 1783, in 1 s. $17^{\circ} 58'$, we have from the place of the node of the axis (that is, 11 s. $17^{\circ} 47'$) to the place of the node of the orbit,

an arch EN of $60^{\circ} 11'$; in the triangle NEO, right-angled at E, there is also given the angle ENO, according to the same author, $1^{\circ} 51'$, which is the inclination of the orbit of Mars to the ecliptic. Hence we find the angle EON $89^{\circ} 5'$, and side ON $60^{\circ} 12'$. Again, when Mars is in the node of its orbit N, we have, by calculation from our principles, the angle PNE = $63^{\circ} 7'$, to which, adding the angle ENO = $1^{\circ} 51'$, we have PNO = $64^{\circ} 58'$; from which two angles PON and PNO with the distance ON, we obtain the inclination of the axis of Mars, and place of its node with respect to that planet's own orbit; the inclination being $61^{\circ} 18'$, and the place of the node of the axis $58^{\circ} 31'$ preceding the intersection of the ecliptic with the orbit of Mars, or in our $19^{\circ} 28'$ of Pisces.

Being thus acquainted with what the inhabitants of Mars will call the obliquity of their ecliptic, and the situation of their equinoctial and solstitial points, we are furnished with the means of calculating the seasons on Mars; and may account, in a manner which I think highly probable, for the remarkable appearances about its polar regions.

But first it may not be improper to give an instance how to resolve any query concerning the martial seasons. Thus, let it be required to compute the declination of the Sun on Mars, June 25, 1781, at midnight of our time. If $\gamma \ 8 \ \pi \ \varpi$, &c. fig. 31. (tab. X.) represent the ecliptic of Mars, and $\gamma \ \varpi \ \triangle \ \nu$ the ecliptic of our planet, Aa, bB, the mutual intersection of the martial and terrestrial ecliptics, then there is given the heliocentric longitude of Mars, $\gamma m = 9 \text{ s. } 10^{\circ} 30'$; then taking away six signs, and $\triangle b$, or $\gamma a = 13. 17^{\circ} 58'$, there remains $bm = 1 \text{ s. } 22^{\circ} 32'$. From this arch, with the given inclination, $1^{\circ} 51'$, of the orbits to each other, we have cosine of inclination to radius, as tangent of bm to tangent of $BM = 1 \text{ s. } 22^{\circ} 33'$. And

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taking away $B\gamma = 1\text{ s. } 1^{\circ} 29'$, which is the complement to νB (or ∞A , already shewn to be $1\text{ s. } 28^{\circ} 31'$) there will remain $\gamma M = 0\text{ s. } 21^{\circ} 4'$, the place of Mars in its own orbit*; that is, on the time abovementioned, the sun's longitude on Mars will be $6\text{ s. } 21^{\circ} 4'$, and the obliquity of the martial ecliptic $28^{\circ} 42'$ being also given, we find, by the usual method, the sun's declination $9^{\circ} 56'$ south.

The analogy between Mars and the earth is, perhaps, by far the greatest in the whole solar system. Their diurnal motion is nearly the same; the obliquity of their respective ecliptics, on which the seasons depend, not very different; of all the superior planets the distance of Mars from the sun is by far the nearest alike to that of the earth: nor will the length of the martial year appear very different from that which we enjoy, when compared to the surprising duration of the years of Jupiter, Saturn, and the *Georgium Sidus*. If, then, we find that the globe we inhabit has its polar regions frozen and covered with mountains of ice and snow, that only partly melt when alternately exposed to the sun, I may well be permitted to surmise that the same causes may probably have the same effect on the globe of Mars; that the bright polar spots are owing to the vivid reflection of light from frozen regions; and that the reduction of those spots is to be ascribed to their being exposed to the sun. In the year 1781, the south polar spot was extremely large, which we might well expect, since that pole had but lately been involved in a whole twelve-month's darkness and absence of the sun; but in 1783 I found it considerably smaller than before, and it decreased continually.

* If no very great accuracy be required, we may add $3\text{ s. } 19^{\circ} 34'$ to any given place of our ecliptic, which will at once reduce it to what it should be called on the orbit of Mars, and will always be true to within a minute.

from

from the 20th of May till about the middle of September, when it seemed to be at a stand. During this last period the south pole had already been above eight months enjoying the benefit of summer, and still continued to receive the sun-beams; though, towards the latter end, in such an oblique direction as to be but little benefited by them. On the other hand, in the year 1781, the north polar spot, which had then been its twelve-month in the sun-shine, and was but lately returning to darkness, appeared small, though undoubtedly increasing in size. Its not being visible in the year 1783 is no objection to these phænomena, being owing to the position of the axis, by which it was removed out of sight; most probably, in the next opposition we shall see it renewed, and of considerable extent and brightness; as, by the position of the axis of Mars, the sun's southern declination will then be no more than $6^{\circ} 25'$ on that planet.

Of the spheroidical figure of Mars.

That a planetary globe, such as Mars, turning on an axis, should be of a spheroidical form, will easily find admittance, when two familiar instances in Jupiter and the earth, as well as the known laws of gravitation and centrifugal force of rotatory bodies, lead the way to the reception of such doctrines. So far from creating difficulties or doubts, it will rather appear singular, that the spheroidical form of this planet, which the following observations will establish, has not already been noticed by former astronomers; and yet, reflecting on the general appearances of Mars, we soon find that opportunities for making observations on its real form cannot be very frequent: for, when it is near enough to view it to an advantage, we see it

6. generally

generally gibbous, and its oppositions are so scarce, and of so short a duration, that in more than two years time we have not above three or four weeks for such observations. Besides, astronomers being already used to see this planet generally distorted, the spheroidical form might easily be overlooked.

Observations relating to the polar flattening of Mars.

1783, Sept 25. 9 h. 50'. I can plainly see that the equatorial diameter of Mars is longer than the polar. Measure of the equatorial diameter $21'' 53'''$; of the polar diameter $21'' 15'''$ *full measure*, that is, certainly not too small. The wires were set as outward tangents to the disk, and the zero, as well as the measures, were taken by the light of Mars.

Sept. 28. 14 h. 25'. I shewed the difference of the polar and equatorial diameters of Mars to Mr. WILSON, Assistant Professor of Astronomy at Glasgow. He saw it perfectly well, so as to be entirely convinced it was not owing to any defect or distortion occasioned by the eye lens; and, because I wished him to be satisfied of the reality of the appearance, while he was observing, I reminded him of several well known precautions; such as causing the planet to pass directly through the center of the field of view, and judging of its figure at the time when it was most distinct and best defined, and so forth.

Sept. 29. I shewed the difference of the polar and equatorial diameters of Mars to Dr. BLAGDEN and Mr. AUBERT. Dr. BLAGDEN not only saw it immediately,

diately, but thought the flattening almost as much as that of Jupiter. Mr. AUBERT also saw it very plainly, so as to entertain no manner of doubt about the appearance.

As we cannot take too many opportunities of confirming our own observations by the eyes of other observers, I esteemed it a very fortunate circumstance to have the honour of a visit from these gentlemen at so particular a time, Mars being this day within 37 hours of the opposition, and yesterday when Mr. WILSON saw it, within about two days and a half.

1783, Sept. 30. 10 h. 52'. The difference in the diameters of Mars is very evident and considerable.

Measure of the equatorial diameter 22'' 9''' with 278.

Second measure - - 22'' 31''' full large.

Polar diameter very exact - 21'' 26'''.

Oct. 1. 10 h. 50'. I took measures of the diameters of Mars with my 20-feet reflector. The equatorial measured 103 parts of the micrometer; the polar 98. The value of the divisions in seconds and thirds not being well determined, on account of some late change in the focal length of the several 20-feet object metals I use, we have only from these measures the proportion of the diameters as 103 to 98.

13 h. 15'. Every circumstance being favourable, I took the following measures of the diameters of Mars with my 7-feet reflector, and a distinct power of 625.

Equatorial diameter 22'' 12''' narrow measure.

22'' 46''' rather full.

22'' 35''' exact.

Polar

Polar diameter $21'' 24'''$

$21'' 33'''$ very exact.

I saw Mars perfectly well all the time I measured, with all its figures upon the disk appearing distinctly; and, I think, these measures may be depended upon better than any I have yet taken.

1783, Oct. 5. 14 h. 0', The difference of the diameters is very sensible.

Oct. 7. 9 h. 43'. The flattening of the poles is very visible.

13 h. 40'. I turned my Newtonian 7-feet reflector one quarter round, so as to bring the place to look in at to the bottom; and, as well as the uneasy posture would allow, I saw the flattening of the poles the same as when I looked in at the side; power 460.

14 h. 30'. With a $3\frac{1}{2}$ feet achromatic telescope and a single eye lens, I saw the difference of the polar and equatorial diameters very plainly.

Oct. 9. 8 h. 40'. I turned my reflector 90° round, so as now to look in at the upper end, but saw not the least difference in appearances; for, returning it again immediately to its usual position, in both cases the equatorial diameter appeared a little longer than the other; power 278, and the evening fine.

I turned the great speculum one quadrant in its cell, but appearances were not in the least altered; the equatorial diameter still was a little longer than the polar one.

I tried a very fine new object speculum, and found also the equatorial diameter a little longer than the polar one.

1783,

1783, Oct. 9. 10 h. 47'. The flattening at the poles very visible.

Oct. 10. 9 h. 55'. A little of the polar flattening is visible, so as to admit of no doubt; power 460, very distinct.

11 h. 32'. Mars visibly flattened, but not much; the achromatic shews it also.

11 h. 42'. The disk of Mars is visibly spheroidical.

Oct. 11. 7 h. 37'. Mars is plainly gibbous, therefore measures and estimations of the diameters must for the future be improper.

11 h. 12'. It is rather difficult to say of what shape Mars is now, for it is partly flattened and partly gibbous; but the gibbous side not being quite in the polar direction of Mars, this produces altogether an odd mixture of shapes: however, upon the whole, the polar diameter is still rather the smallest.

11 h. 13'. The *preceding* side of Mars shews the flattening of the poles, while the *following* is terminated by an elliptical arch.

Oct. 12. 11 h. 12'. The flattening upon the whole is visible.

Oct. 17. 13 h. 7'. The effect of gibbosity is scarcely equal to the flattening; or, upon the whole, the planet is still rather broader over the equator than over the poles.

Nov. 1. 7 h. 56'. The semi-disk, which is *full*, is evidently part of an oblate spheroid; but, to an eye not attentively looking for it, and knowing the shape and exact situation of the poles of Mars, this would probably not appear.

1783, Nov. 10. 9 h. 30'. The gibbosity of Mars is now such, that the polar diameter is considerably longer than the equatorial; but the deficiency not being exactly from pole to pole, makes the disk of a crooked, irregular figure, and renders precision in this estimation impossible; otherwise the phase of Mars would have made a pretty good micrometer upon the equatorial diameter, and it was with such a view I had directed my attention to this circumstance: appearances, however, are visibly in favour of the polar diameter's being the longest.

We find that the quick alterations in the visible disk of Mars, during the time it is in the best situation for us to observe it, are such, that if we were to use many measures which have been taken of its diameters, we should be obliged to have recourse to a computation of its phases, in order to make proper allowance for them. Now, since these changes are in a longitudinal direction, and the poles of Mars are not perpendicular to the ecliptic, it would bring on a calculation of small quantities, which it is always best not to run into where it can be avoided. For this reason, I shall at once settle the proportion of the equatorial to the polar diameter of this planet, from the measures which were taken on the very day of the opposition. I prefer them also on another account, which is, that they were made in a very fine, clear air, and were repeated with a very high power, and with two different instruments, of whose faithful representation of celestial objects, the many observations on very close double stars I have made with them have given me very evident proofs.

As

As we are at present only in quest of the proportion of one diameter to the other, the measures of the 20-feet reflector, though not given in angular quantities, will equally suffice for the purpose. By them we have the equatorial diameter to the polar as 103 to 98, or as 1355 to 1289. I have turned the proportion into the latter numbers by way of comparing them the better with the measures of the 7-feet reflector. By that instrument the equator of Mars, Oct. 1. we find, was measured three times; but from the remarks annexed to the different results, I think the third measure should be used. Indeed, on taking the difference of the two first, which is $34''$, and dividing by three, we have the quotient $11\frac{1}{3}''$; then, allotting two-thirds to the first, because the remark says positively "narrow measure," it becomes $22'' 34\frac{1}{3}''$, and taking one-third from the second, which is expressed doubtfully, "rather too full," it becomes $22'' 35\frac{1}{3}''$: this reflection on the two first measures gives additional validity to the third, which is $22'' 35''$, or $1355''$. The polar diameter was measured twice; and as no reason appears against either of the observations, I shall take the mean of both, which is $21'' 29''$, or $1289''$; so that by these measures the equatorial diameter of Mars is to the polar as 1355 to 1289. A less perfect agreement between the proportions of the diameters arising from the measures of the 20-feet reflector and those which we have just now deduced from the 7-feet, would have been sufficient for our purpose, as we might easily have excused one or two thousandths of the whole quantity; however, we have no cause to be displeased with this coincidence, though it should in part be owing to accident, and therefore shall admit the above proportion, and proceed to a farther examination of it.

In the first place, it will be necessary to see whether any correction be required on account of the different heliocentric and geocentric south latitude of Mars; which would apparently compress the polar diameter a little, by the defect of illumination on the north. On computation we find, that a difference arising from that cause would give the longitudinal diameter to the latitudinal as 20000 to 19987; which being much less than one thousandth part of the whole, may therefore be neglected.

But next, a very considerable correction must be admitted, when we take into account the position of the axis of Mars. The declination of the sun on that planet, at the time the measures were taken, was not less than 27° south; so that the poles were not in the circumference of the disk by all that quantity. On a supposition then, that the figure of Mars is an elliptical spheroid, we are now to find the real quantity of the polar diameter from the apparent one. It has been proved, that, in the ellipsis, the excesses of any diameters above the polar one are as the squares of the cosines of the latitudes*; but the diameter at rectangles to the equator of Mars, which was exposed to our view in the late opposition, was not the polar one, but such as must take place in a latitude of 63° . Putting therefore $m = \text{cosine of } 63^{\circ}$, $a = 1355$, $b = 1289$, $x =$ the polar axis, we have $1 : m^2 :: a - x : b - x$. And $\frac{b - m^2 a}{1 - m^2} = x$; which gives us 1272 nearly, for the polar diameter. The true proportion, therefore, of the equatorial to the polar diameter will be as 1355 to 1272; which, reduced to smaller but less accurate numbers, is 16 to 15 nearly.

* Astr. par M. DE LA LANDE, § 2680.

I shall

I shall now also mention some of the other measures, but with a view only to shew that they are very consistent with the above determination. From those of the 30th of September, for instance, we collect the proportion of the diameters of Mars as 1340 to 1286; or, reduced to our former numbers, 1355 to 1300. Now, since these measures were taken the night before the opposition, they must on that account be as good as the former; and, had those of the day of opposition not been preferred, because they were oftener repeated, and the superior power of the 7, and great light of the 20-foot reflector, gave them additional weight, I should have taken them into the account; the very small difference, however, cannot but strengthen the results of the former measures.

From the observations of the 25th of September we have the proportion of the diameters as 1313 to 1275; and if the equatorial measure be increased in the ratio of 20000 to 19953, on account of the different heliocentric and geocentric longitude, Mars not being at the full, it will give the ratio of 1316 to 1275; or, conforming to our former numbers, as 1355 to 1312. I have not been very strict in the application of the correction deduced from the phases of Mars, since no other use was intended to be made of these numbers than merely to shew, that they do not very greatly differ from those we have assigned before *.

It.

* If more strictness be required, let EC, fig. 32. be the ecliptic; PS its poles; *p* the poles of Mars, and *eq* its equator. Then, the angle *p**m*C being found, by calculation, we shall have *Cm* (radius) to *cm* (cosine of the difference between the heliocentric and geocentric longitude) as *qv* (sine of the angle *qmv* or *pmC*) to *ov*. Then, since with Mars *Cc* can never be very great, the small triangle *qno* may be taken for similar to *qvm*; therefore *qm* (radius) is to *qv* (sine of *pmC*).

It was observed, Oct. 17, 1783, that the equatorial diameter of Mars was still greater than the polar, notwithstanding the depredation of the defect of light upon it. On calculating the phases, we find, that the longitudinal diameter was, that day, to the latitudinal one as 19711 to 20000, which therefore could not be an equal balance to oppose the spheroidal figure so as to render it invisible.

But, Nov. 10. the proportion of the longitudinal diameter to the latitudinal one, from a computation of the phase of Mars, must have been as 18762 to 20000; and accordingly it was by observation found to be more than sufficient to take off all appearance of the polar flattening, and leave a visible excess in the axis above the equator.

To obviate any doubts concerning a fallacy that might arise from the convexity of the eye-glass, or irregular shape of the small speculum, I need only refer, for the latter, to the experiments of the 7th and 9th of October, 1783: for should the short diameter of my small plane speculum have occasioned a compressing of the polar diameter of Mars when exposed to it, half a turn of the telescope must bring the other diameter of that speculum into the same situation, and a contrary effect would have followed. With regard to the former, not only the experiments made with the achromatic, but principally the observation with the 20-feet reflector, where I used a compound eye-piece magnifying only about 300 times, will sufficiently exculpate the eye-glasses. It is also well known, that in a single lens the distortion of the images, if any such there

$p m C$) as $q o (= q v - v o)$ to $q n$; which is the required correction or deficiency of the equatorial diameter $e q$ of Mars.

Or, putting $m C \approx 1$ and $v q \approx m \cos$ of the angle $P m p$; it will be $q n \approx m^2 \cdot c C$.

should be, will equally affect the wires of the micrometer, and give a true measure notwithstanding; and the compound eye-piece I used with the 20-feet reflector had likewise the same advantage, for it is constructed on the plan lately proposed by Mr. RAMSDEN in the Philosophical Transactions*, which he was so obliging as to communicate to me about a twelve-month ago, and which I immediately adapted to my large micrometers.

On the subject of the figure of Mars I ought to remark also, that perhaps the measures which were taken of its diameters during the last opposition will enable us to ascertain its real size with greater accuracy than has been done before. The micrometer which can distinguish with precision between the equatorial and polar diameters of this small planet, will certainly be admitted as an evidence of considerable consequence; and since the result of these measures is pretty different from what former observations give us, I should not omit mentioning it.

We have seen that the equatorial diameter, on the day of the opposition, measured $22'' 35'''$. The distance of Mars from the earth at that time was .40457, the mean distance of the earth from the sun being 1; therefore, $22'' 35''$ reduced to the same distance will be no more than $9'' 8'''$.

I shall conclude this subject with a consideration relating to the atmosphere of Mars. Dr. SMITH † reports an observation of CASSINI's, where "a star in the water of Aquarius, at the distance of six minutes from the disk of Mars, became so faint before its occultation, that it could not be seen by the naked eye, nor with a 3-feet telescope." It is not men-

* Vol. LXXIII. p. 94.

† Optics, § 1096.

tioned what was the magnitude of the star; but, from the circumstance of its becoming invisible to the naked eye, we may conclude, that it must have been of the sixth or seventh magnitude at least. The result of this observation would indicate an atmosphere of such an extraordinary extent, since at the distance of 36 semi-diameters of the planet it should still be dense enough to render so considerable a star invisible, that it will certainly not be amiss to give an observation or two which seem of a very different import.

1783, Oct 26. There are two small fixed stars preceding Mars, of different sizes; with 460 they appear both dusky red, and are pretty unequal; with 278 they appear considerably unequal. The distance from Mars of the nearest, which is also the largest, with 227 measured $3' 26'' 20'''$. Some time after, the same evening, the distance was $3' 8'' 55'''$, Mars being retrograde. I saw them both very distinctly. I viewed the two stars with a new 20-feet reflector of 18.7 inches aperture, and found them, as I expected, very bright.

Oct. 27. I see the two small stars again. The small one is not quite so bright in proportion to the large one as it was last night, being a good deal nearer to Mars, which is now on the side of the small star; but when I draw the planet aside, or out of view, I see it then as well as I did last night. The distance of the small star measured $2' 56'' 25'''$ *.

* The measures were accurate enough for the purpose, though not otherwise to be depended on nearer than, perhaps, six or eight seconds.

X. p. 272.

Page 2c.

The largest of the two stars on which the above observations were made cannot exceed the twelfth, and the smallest the thirteenth or fourteenth magnitude; and I have no reason to suppose that they were any otherwise affected by the approach of Mars, than what the brightness of its superior light may account for. From other phenomena it appears, however, that this planet is not without a considerable atmosphere; for, besides the permanent spots on its surface, I have often noticed occasional changes of partial bright belts, as in fig. 1. and 14.; and also once a darkish one, in a pretty high latitude, as in fig. 18. And these alterations we can hardly ascribe to any other cause than the variable disposition of clouds and vapours floating in the atmosphere of that planet.

Result of the contents of this paper.

The axis of Mars is inclined to the ecliptic $59^{\circ} 42'$.

The node of the axis is in $17^{\circ} 47'$ of Pisces.

The obliquity of the ecliptic on the globe of Mars is $28^{\circ} 42'$.

The point Aries on the martial ecliptic answers to our $19^{\circ} 28'$ of Sagittarius.

The figure of Mars is that of an oblate spheroid, whose equatorial diameter is to the polar one as 1355 to 1272, or as 16 to 15 nearly.

The equatorial diameter of Mars, reduced to the mean distance of the earth from the sun, is $9'' 8'''$.

And that planet has a considerable but moderate atmosphere, so that its inhabitants probably enjoy a situation in many respects similar to ours.

Datchet, Dec. 1, 1783.

W. HERSCHEL.

VOL. LXXIV.

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XX. *A Description of the Teeth of the Anarrhichas Lupus Linnæi, and of those of the Chætodon nigricans of the same Author; to which is added, an Attempt to prove that the Teeth of cartilaginous Fishes are perpetually renewed. By Mr. William Andre, Surgeon; communicated by Sir Joseph Banks, Bart. P. R. S.*

Read March 18, 1784.

THE amazing variety there is in the external form of fishes must be obvious to a common observer; and whoever examines will be convinced, that the same variety prevails in their internal structure. No parts, perhaps, afford a more convincing proof of the last assertion than the teeth of fishes. To adduce a few instances, let us only recollect the tuberculated teeth in the thorn-back; the triangular serrated teeth in the shark; the slender flexible teeth in the *chætodontes*, or angel-fishes. There is not only a difference of their form, but also in the substances of which they are composed; some being of a soft horny nature; others made up of bone; others of that substance we call enamel in the teeth of quadrupeds; and some having the apparent hardness and transparency of crystal. We may also notice their uncommon situation; many fishes having teeth not only in their jaws, but on the tongue, the palate, and about the *fauces*.

To illustrate in some degree this part of natural history, I shall describe the teeth of the *Anarrhichas Lupus*, or Sea-wolf, and those of the *Chætodon nigricans*, a species of Angel-

Angel-fish. The former have been but imperfectly described, and never represented distinct from the fish, without which it is impossible to have any exact idea of their disposition, number, or form, while the true shape and composition of the latter, from their minuteness, have been entirely overlooked. I shall then attempt to prove, that a continual renovation of the teeth takes place in cartilaginous fishes.

THE SEA-WOLF is a fierce and ravenous fish, as its name imports, found in the northern parts of the globe, where it frequently grows to the length of four feet and upwards.

The jaws of the Wolf-fish are made up of several bones, to each of which a greater or less number of teeth are affixed; but, before I enter upon the description of them, I shall take notice of the palate (marked A. tab. XI.), that being a kind of basis or support to the other bones, to which they are all more or less connected. The palate is a thick and firm bone united above to the bones of the *cranium* and nose, and ending below in a flat oval surface, on which are incrustated about twelve or thirteen strong, blunt, and rather flat teeth of the *molar* or grinder kind. The external edges of the teeth are the most prominent; by which means a hollow is formed in the middle of the palate.

The upper jaw is composed of three bones, two of which (BB) are placed laterally, forming the sides of the upper jaw, and the third (C) anteriorly, making the fore-part of the jaw. The third bone may be divided through its middle into two portions; but since it has the appearance of one bone only, the connection being very firm, I shall describe it agreeably to that appearance, to prevent needless divisions.

The side bones of the upper jaw have nearly the shape of an italic *f*. At their posterior ends may be observed a smooth

articular surface, for their connection with a similar surface on the posterior extremities of the lower jaw; and on their anterior ends there are two rows of teeth. The external row consists of three or four sharp or conical teeth; and the internal row of four or five blunt and rather flat ones. These bones are connected to the palate and bones of the nose by loose but strong ligaments.

The third bone of the upper jaw, which may be called the anterior or nasal portion, is of a triangular form, connected above to the bones of the nose, and ending below in a flat surface, thick-set with sharp conical teeth. The external teeth, about four in number, are large and strong, and bend a little inwards; but the internal ones are small, and nearly straight, of which we may reckon about ten.

This bone is connected above (as I have before observed) to the bones of the nose; between which a complete joint is formed, of that kind called by anatomists *ginglymus*, that is, where the projecting parts of one bone are received by corresponding cavities in the other. Like other articulations, it is furnished with a capsular ligament, and no doubt an apparatus for the secretion of *synovia*. Although a joint exists between this bone, and those of the nose, yet no muscles are provided for its motion, which depends entirely upon the resistance made by those hard bodies which the animal takes into its mouth.

The lower jaw (D) consists of two bones, united at their fore-parts by a strong ligament, which allows of some motion. On their anterior extremities are placed six large and as many small sharp and conical teeth; the large teeth are placed externally, and their points are bent a little inwards; while the small ones, which stand within them, are nearly straight. Behind these are two or three rows of grinder teeth. The
external

external teeth stand nearly upright; but the internal ones are placed obliquely, inclining towards each other.

The teeth are formed of a hard bony matter, not covered with enamel as in some animals; nor is there an equal distribution of enamel and bone as in some others. They are not fixed in sockets, but are fastened to the jaws in the same manner as the *epiphyses* are united to the bodies of the bones in young animals.

From the foregoing description it will appear, that the anterior sharp teeth of the Sea-wolf are admirably calculated for seizing its prey, while the posterior grinding teeth serve to break down the hard shells of lobsters, crabs, muscles, scollops, &c. which this animal is known to feed upon. The external teeth on the sides of the upper and lower jaw being higher than those placed within them, a hollow is formed above and below, in which the convex shells of crustaceous animals, &c. are confined during their compression between the jaws, which is effected by the action of strong muscles placed on the sides of the head. The jaws being made up of a number of pieces, and connected by loose ligaments, a freedom of motion is allowed, and the collision or shock arising from the comminution of hard bodies is so much the less by being divided among a number of bones.

MERRET informs us *, the *lapis busonites* are the flat grinder teeth of this fish petrified. But certainly these fossils are not the production of the Sea-wolf alone, since they may originate from all those fishes which have flat teeth in their palate or jaws; a structure which the French naturalists distinguish by the appellation of *palais pavé*.

* Pinax Rerum Naturalium Britannicarum.

OF THE CHÆTODON NIGRICANS.

The individual which furnished the following account was brought from the West Indies, and measured about five inches in length*. Its teeth (the only parts I mean to describe) were so small as to require the assistance of a microscope to discover their real shape. There were fourteen teeth in each jaw, seven of which from the upper one are represented tab. XII. They consist of a cylindrical body fixed in the jaw, above which they spread out into a broad and rather flat surface, on the edges of which are twelve or thirteen *denticuli*, making an uncommon appearance, and totally different from the teeth of any other animal. Another singularity is their being transparent, unless viewed with a deep magnifier, when a few opaque lines may be perceived, which point out the cellular part of the tooth through which the blood vessels ramify, which are destined for its growth and nourishment. They are not all of the same length. Those in the anterior parts of the jaws are the longest, from whence they gradually diminish in length as they approach the angles of the mouth.

From the foregoing description of the teeth of the *Chætodon nigricans*, this fish seems to be misplaced in the *Système Naturel* of LINNÆUS; since one generic distinction of the *Chætodontes* is to have numerous, slender, and flexible teeth; whereas the teeth of the *Chætodon nigricans* are few in number, placed in one row, and of a crystalline hardness.

* This fish is well represented in DU HAMEL *Traité général des Pêches*, tom. III. seconde partie, section IV. planche xiii. under the name of *Chirurgien* au *Porte Lancette*.

OF THE TEETH OF CARTILAGINOUS FISHES.

When STENO examined the teeth of the shark, he was surprised to find a great number of them placed on the inside of each jaw, lying close to the bone, and many of them buried in a loose spongy flesh; concluding that these internal teeth could be of little or no use to the animal. Mr. HERISSANT * afterwards shewed the use of these internal or posterior teeth, by proving, that as the anterior teeth of each row are broken off, drop out, or wear away, the posterior ones come forward to supply their places †.

But though it be certain that the anterior teeth, when lost, are replaced by the posterior ones, neither of the above naturalists, or any other that I know of, have attempted to ascertain how often this circumstance happens. Whether the renovation be perpetual during life; or whether that operation be suspended after a limited number of teeth have been supplied.

From a singular circumstance, which I met with some time ago, I am inclined to think the former is the fact; or, that in cartilaginous fishes, such as sharks, rays, &c. there is a perpetual renovation of the teeth.

Being engaged in dissecting the jaws of a very large shark, I was surprised to find a portion of that sharp, bearded bone found in the tail of the fire-flare, or sting-ray ‡, driven quite

* BOMARE Dictionnaire d'Histoire Naturelle, article Requien.

† It may not be improper on this occasion to point out a mistake which some naturalists have fallen into, in allowing a set of muscles for raising the numerous teeth placed in the jaws of sharks. I have frequently dissected the jaws of these animals, and am certain no such muscles exist, nor are they indeed at all necessary.

‡ *Raja Pasipaca* LINNÆI. The French naturalists, on account of the bone in the tail, call this fish *Raie bonnette*.

through

through the lower jaw among the posterior teeth, and fixed almost immoveably. How this happened must be obvious to every one. (See the figure, tab. XIII.)

Before I proceed, it will be necessary to observe, first, that the posterior teeth of cartilaginous fishes are always found in a soft, membranous state, and but imperfectly formed; notwithstanding this, they have the whiteness of teeth from a small quantity of calcareous earth already deposited within their substance. Their hardness and perfect form is acquired as they advance towards the anterior parts of the jaws. Secondly, that of the three angles in each tooth of the shark, one is placed towards the right, another towards the left, and the other, which is in the middle, and the most acute angle, is directed inwardly towards the tongue or *fauces*. They are placed then in such a manner as that the angles of the teeth on the left-side in one row, approach the angles of the teeth on the right-side in the next row. Those teeth which stand on a line from without inwards, I call a row; not those which are placed nearly in a parallel line from one side of the mouth to the other.

The sharp bone of the sting-ray was fixed in the lower jaw between two rows of teeth; and at their posterior part, where the first rudiments of the future teeth are formed, and it will be clear to every one, particularly those who are conversant in such matters, that this could not have happened without producing a great deal of pain, swelling, and disorder in the part where it was fixed. It is unnecessary to enumerate the different kinds of mischief this might occasion. Let it suffice to observe, that on account of the space taken up by this extraneous body, the teeth on each side of it, for want of room, could never after be perfectly formed. The teeth on the left-side
wanting

wanting their angles to the right, and the teeth on the right-side being destitute of their angles to the left.

As it is certain, that the anterior teeth were formerly posterior ones, and as the teeth in each row were all deficient in one angle, it follows, that they must have been formed posterior to the insertion of this extraneous body. Again, if we allow that before the accident the animal was in possession of perfect teeth, it follows also, that they were consumed and replaced by imperfect ones.

There were six teeth in each row, and fifty-two rows, making together about 312 teeth. Now allowing the consumption to have been equal in all parts of the jaws, it follows, that the animal had already consumed 312 teeth, and was in possession of a like number for future consumption.

The teeth of sharks, rays, &c. may be divided into active and passive. The active teeth are the anterior ones of each row, standing with their points upwards. The passive teeth are the remaining ones, lying one upon another, like the tiles upon a house (imbricated), with their points downwards. It appears from the foregoing account, that the anterior or active teeth had been replaced six times; and that they might have been renewed six times more, making in all twelve times. From which, I think, we may reasonably conclude, that this does not happen any precise number of times; but that the renovation is perpetual during the life of the animal.

The longevity of fishes is a fact pretty well established. In addition to this part of natural knowledge, I have endeavoured to prove, that a part of the inhabitants of the great deep retain, in the article of teeth, a perpetual juvenility, being apparently utter strangers to edentulous old age.

EXPLANATION OF THE PLATES.

Tab. XI. The jaws of the Wolf-fish.

A. The palate.

BB. The side bones of the upper jaw.

C. The anterior or nasal portion of the same.

D. The lower jaw.

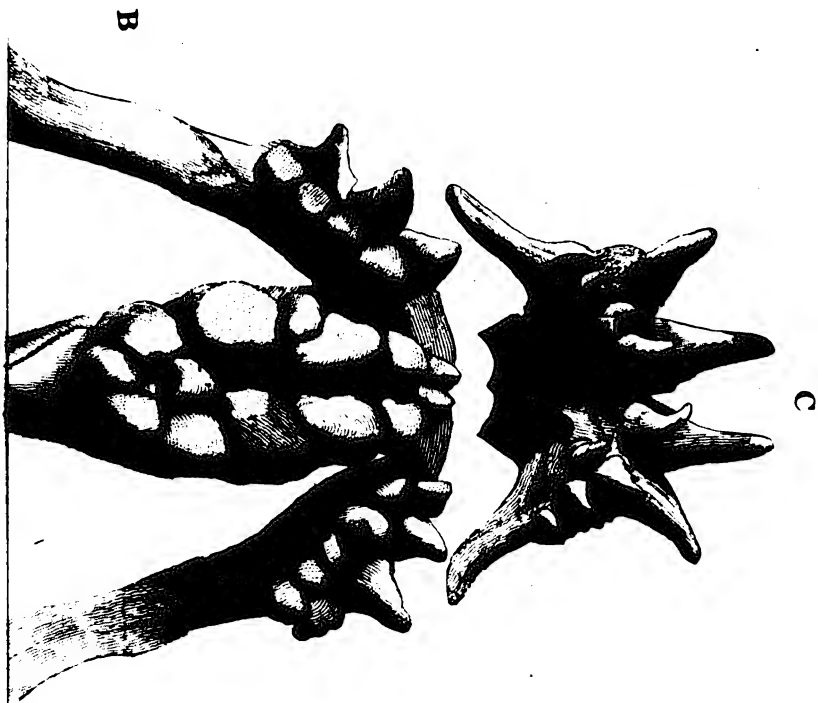
Tab. XII. The teeth of the *Chætodon nigricans* magnified.

Tab. XIII. Part of the lower jaw of a large shark.

AA. Two rows of perfect teeth.

BB. Two rows of imperfect ones.

C. The bone of the sting-ray.



Philos. Trans. Vol. LXXIV. Tab. XII. p. 282.



XXI. *Abstract of a Register of the Barometer, Thermometer, and Rain, at Lyndon, in Rutland, 1783. By Thomas Barker, Esq.; communicated by Thomas White, Esq. F. R. S.*

Read March 18, 1784.

The year began with a short dry frost, then showery, intermixed with frost. The end of January, and near half of February, stormy and wet, and after ten days fine and mild; a severe season for snow, wet, wind, and frost. The end of February and beginning of March cut the grafs, corn, and stock, more than all the winter before. From March 10. to May 27. was a very dry season and fine seed-time; but so dry at last the late sown corn could not come up. The spring was pleasant; but almost constant frosty mornings till April, and frequent afterward, kept things backward; and though there was some fine warm weather the middle of April, yet later in May the drought and N.E. winds stopped the growth of things; and two sharp frosty nights, May 25. and 26. the rime was so particularly cutting in the meadows, that the young shoots on many oak and ash trees in the vallies were entirely killed, while those on the hills were unhurt, and some of the tops of the trees escaped, though the bottoms were blasted.

May 27. to 30. in a continued three days rain there fell $3\frac{1}{2}$ inches, which is, I believe, the most that has come in one continued unceasing rain since July 1736, when, in about the same time, there came five inches; but the rain this May was not alike in all places, for there was not a quarter so much in Hampshire. This rain was of vast service to bring up the late sown corn, and make the grafs grow well; but this and some other hasty rains afterwards hurt the meadow grafs, by flooding it three times. Hot weather succeeding, it was a very growing time, and ten days together, in the middle of June, were all wet.

During the showery time an uncommon haziness began, which was very remarkable all the rest of the summer: the air was all thick both below the clouds and above them, the hills

hills looked blue, and at a distance could not be seen; the sun shone very red through the haze, and sometimes could not be seen when near setting. There was more or less of this haze almost constantly for a month, and very frequently to the end of the summer, and it did not cease till Michaelmas; and neither rain nor fair, wind nor calm, east nor west winds, took it away; and it was as extensive as common, for it was the same all over Europe, and even to the top of the Alps. This haze was very like VIRGIL's description of the summer after J. CÆSAR's death, which was probably the same case,

Cum caput obscurâ nitidum ferrugine texit,

for rusty iron is a very good description of the colour the sun shone. But by PLUTARCH's account, near the end of C. CÆSAR, that summer was very different from this in other respects for, he says, the sun gave very little heat, the air was cloudy and heavy, and the fruits not ripened, which was not the case this year; for this was a dry haze, the summer in general hot and dry, and in some countries very much so.

I think I never knew more mischief done by thunder than there was in different places this year, from the beginning of July, and very seldom more or hotter weather; yet where they had not those thunder-showers they suffered by being burnt up. Here we never wanted grass after May, and the hay and harvest were both well got in, but in Surrey, Hampshire, and Dorset, they were very much burnt up, had little hay, and as they had a good deal of showery weather in harvest, their barley suffered twice, from not coming up in time, and again in getting it in. As the rain this year was chiefly in showers or sudden rains, it fell very uncertainly, as appears by
comparing;

comparing what fell here with that in Hampshire. The latter part of August and first half of September was showery; but in this country not so much as to hurt the harvest, a great part of which was in first. The crop of grain was in general pretty good, but did not yield enough to make up the defect of the last year's crop, every body was so much out of all sorts, as the corn last year was both scarce and bad; grain, therefore, continued dear this year, especially barley.

The summer 1782 had been so cold and wet, that the flower buds on many trees were very small and not perfected, so that this spring there was a great want of blossoms on the wall fruit and apples, and exceeding few indeed on ash-trees and hawthorn. I do not know of any ashkeys at all, nor any bunches of haws, only a few scattered single ones; but cherries and plumbs blossomed well, and there was no want of fruit; plenty of currants, and vast quantities of gooseberries.

August 18. a remarkable ball of fire was seen between nine and ten at night all over England, and even in foreign countries. It seemed to move from north to south or south-east. There was another October 4. but not so much observed, and some say another afterward, but little seen; but there were very few northern lights this autumn.

The autumn was a very fine one; calm, fair, and mild, but rather too dry for the sowing of wheat, which, however, in general came up well, and what lay dry was brought up very finely by ten days wet the middle of November; after which it was dry and fine again, an open mild time, with few frosty mornings; but a good deal of dark or misty weather in December, yet mild till the last week, when there came a great snow, very severe frost, and cutting strong wind, which ended the year.



XXII. *On the Periods of the Changes of Light in the Star Algol. In a Letter from John Goodricke, Esq. to the Rev. Anthony Shepherd, D. D. F. R. S. Professor of Astronomy at Cambridge.*

Read April 1, 1784.

SIR,

York, Dec. 8, 1783.

AS I am now able, by collating some of my late observations on Algol with those I sent you last May, to determine with greater precision the periodical return of its changes, I wish to add this as a kind of supplement to that account.

The method I have here pursued is by taking the intervals between accurate observations of Algol's least brightness or greatest diminution of light made at long distances of time from each other, and dividing those intervals by a certain number of revolutions, as will be best understood by the table below. The reason of my chusing long intervals is; that the number of revolutions being greater, the errors of observation are thereby diminished: all error cannot, however, as yet be excluded, but I think the period is now, by the following calculation, ascertained within ten or fifteen seconds.

Mean times of Algol's
least brightness.

1783	h. m. s.						d. h. m. s.
Jan. 14	9	25	39	} an interval of 99 revolutions, each of	2	20	49 14
Oct. 25	6	39					
Jan. 14	9	25		} Ditto 106	Ditto	2	20 49 10
Nov. 14	8	17					
Jan. 14	9	25		} Ditto 107	Ditto	2	20 49 2
Nov. 17	4	52					
Feb. 6	8	15		} Ditto 91	Ditto	2	20 49 3
Oct. 25	6	39					
Feb. 6	8	15		} Ditto 98	Ditto	2	20 48 59
Nov. 14	8	17					
Feb. 6	8	15		} Ditto 99	Ditto	2	20 48 51
Nov. 17	4	52					
Feb. 26	9	43		} Ditto 84	Ditto	2	20 49 14
Oct. 25	6	39					
Feb. 26	9	43		} Ditto 91	Ditto	2	20 49 9
Nov. 14	8	17					
Feb. 26	9	43		} Ditto 92	Ditto	2	20 49 0
Nov. 17	4	52					
Jan. 31	14	29		} Ditto 100	Ditto	2	20 49 4
Nov. 14	8	17					
Mar. 21	8	36		} Ditto 84	Ditto	2	20 48 46
Nov. 17	4	52					

Hence the period of Algol's variation is, on a mean,

2 20 49 13

I could have added several more comparisons of the like kind; but these are, I think, sufficient. It is to be remembered, that all the observations contained in the above table are reduced to mean time.

It appears to me now, that the duration of the variation is about eight hours; but, as it is difficult to hit exactly the beginning and end of the variation, this may occasion different observers to differ in this respect. Before I conclude, I beg leave to mention a circumstance deserving of notice; which is, that

that FLAMSTEAD has also amongst other stars observed Algol, and in two places has marked it of less magnitude than at other times, *viz.* of the third magnitude, 1696, January 16. 6 h. 24', and 1711, December 5. 9 h. 13', both mean time and old stile *. Suspecting these might probably be days of Algol's variation, I computed the interval between them, but could not find a period answerable to that which I have above determined. Upon examining more closely the observations, I find, in that of 1696, he marked at the same time the magnitude of ϵ Persei; which, considering especially the nearness of ϵ Persei to Algol, makes this observation to be relied on for its justness, and less liable to any mistake of judgement; whereas the other observation of December 5, 1711, is more liable to error or doubtfulness, because he did not then mark the magnitude of ϵ Persei, or of any star of the same magnitude near enough to Algol. Presuming, therefore, on the justness of FLAMSTEAD's observation of 1696, to think that it probably was made at a time when Algol varied, I compared it with one of mine, *viz.* October 25. 6 h. 39', 1783, and I find there is, in the interval between those observations, either 11,176 periods, each of 2 d. 20 h. 49' 18"; or 11,177, each of 2 d. 20 h. 48' 56". The last, as it approaches nearest to the results of my best observations, I think, is the exactest determination of the period. This, however, all proceeds upon the supposition that Algol varied at the time of FLAMSTEAD's observation, and also that the period is regular.

* *Historia Cœlestis*, vol. II. edit 1725, p. 284. and 534.

The following is a short abstract of my late observations on Algol, when its least magnitude was accurately determined.

August 17, 1783.

App. time.

h. m.

- 10 52 About equal to ϵ Persei, though Algol seemed to be rather brighter.
 11 7 Evidently less than ϵ Persei.
 11 22 Ditto; but rather difficult to distinguish them from each other.
 11 30 Rather brighter than ϵ , and not so bright as δ Persei.
 12 0 About the brightness of δ Persei, and rather less than β Trianguli.
 12 30 Brighter than δ Persei, and rather not so bright as β Trianguli.

From those observations, by taking a mean between 11 h. 7' and 11 h. 22', it appears, that its least brightness happened at 11 h. 14'; true, I think, to 5'.

October 25.

- 6 40 It was considerably less than ϵ Persei.
 7 5 Ditto.
 7 20 Equal to ϵ Persei, though Algol seemed rather less.
 7 35 About equal to ϵ Persei.
 7 50 Brighter than ϵ , and also than δ Persei.
 8 25 About the third magnitude, and equal to β Trianguli.
 9 35 Between the second and third magnitude; brighter than β Arietis, and rather less than α Pegasi.
 10 10 About the second magnitude; rather brighter than α Pegasi, rather less than β Cassiopeæ, and not so bright as α and γ Cassiopeæ.

Rather

App. time.

- h. '
 10 40 Rather brighter than β Cassiopeæ, but less than α and γ .
 11 0 Nearly equal to, if not rather brighter than, γ Cassiopeæ, and less than α Cassiopeæ*.

In 20' afterwards it was of the same brightness; hence we may conclude, that the variation has ended at 11 h. 0'.

Its least brightness from the observations appears to have happened at 6 h. 55'; true, I think, to 10'.

November 11.

- 10 5 Third magnitude; not much different from ϵ Persei and β Trianguli.

- 10 45 Between the third and fourth magnitude; believe equal to δ Persei.

- 11 14 Less than ϵ Persei.

- 11 48 Ditto; but think it rather increased.

Its least brightness from those observations appears to have happened at 11 h. 31'; true, I believe, to a quarter of an hour. The weather was rather hazy.

November 14.

- 5 0 Between the second and third magnitude, and less than β Cassiopeæ.

- 5 45 A little brighter than β Arietis.

- 6 50 Not so bright as β Arietis, and rather brighter than β Trianguli.

- 8 10 A little brighter than ϵ Persei, and believe equal to δ Persei.

* Algol's usual and greatest brightness, by my later and more accurate observations, is thus: a little less than α Cassiopeæ, brighter than β Cassiopeæ and α Pegasi, and rather a little brighter than γ Cassiopeæ.

Q q 2

Less

App. time.

h.

8 25 Less than ρ Persei.

8 40 Ditto.

9 0 Equal to ρ , though Algol appeared rather brighter.9 15 A little brighter than δ and ρ Persei.

By taking a mean between 8 h. 25' and 8 h. 40', it appears, its least brightness happened at 8 h. 32'; true to 10 minutes. The weather was rather hazy during some part of this observation.

November 17.

4 58 A little less than ρ Persei.

5 15 Ditto.

5 35 Rather brighter than ρ Persei.5 50 A little brighter than ρ Persei, but less than δ Persei.6 5 Rather brighter than δ Persei.6 40 Equal to β Trianguli, and brighter than ϵ and ζ Persei.7 20 A little brighter than β Arietis.8 30 Between the second and third magnitude, and equal to β Cassiopeæ, but less than α and γ .8 50 Second magnitude, and equal to γ Cassiopeæ.

9 25 Nearly the same, if not rather brighter.

The variation has therefore ended at 9 h. 0' nearly, and its least brightness by taking a mean between 4 h. 58' and 5 h. 15', happened at 5 h. 7'; true, I believe, to 10 minutes. The weather was fine.

I have several more observations on Algol, where I have not been able to ascertain its least brightness, which all happened agreeable to the period as above determined; viz. May 20. July 5. and 22. August 14. September 6. 9. 12. and 26. October 2. 5. 19. and 22. and December 7.



XXIII. *Experiments and Observations on the Terra Ponderosa, &c.* By William Withering, M. D.; communicated by Richard Kirwan, Esq. F. R. S.

Read April 22, 1784.

S E C T I O N I.

Terra ponderosa aërata.

THIS substance was got out of a lead-mine at Alston-Moor, in Cumberland. I first saw it in the valuable collection of my worthy and ingenious friend MATTHEW BOULTON, Esq. at Soho; who, when he picked it up, conjectured from its weight that it contained something metallic. About two years ago I saw it in his possession; and partly from its appearance, being different from that of any calcareous spar I had seen, and partly from its great weight, I suspected it to be the spatum ponderosum.

A few experiments made at the moment confirmed my suspicions, at least so far as to shew that it contained a large proportion of the terra ponderosa united to fixed air; but I did not then flatter myself that it would prove so pure as I afterwards found it to be.

Professor

Professors BERGMAN, in his *Sciagraphia Regni Mineralis*, published last year at Leipzig, conjectures (§ 58.), with his usual sagacity, "*Terra ponderosa nitrata forte alicubi nativa occurrit, a nomine tamen adhuc inuenta, quod etiam valet de aerata.*"

I was much delighted by the detection of a substance which promises to be of very considerable utility in chemical inquiries, and more so when I found it to be a native of this country; for it is not improbable, that it may be met with in many other mines, besides that at Alston-Moor.

Mr. BOULTON, with his usual benevolence, presented me with a piece of it, part of which accompanies this paper, for the inspection of the Members of the Royal Society.

More obvious Properties.

Its general appearance is not much unlike that of a lump of alum; but, upon closer inspection, it seems to be composed of slender spiculae in close contact, but more or less diverging. It may be cut with a knife. Its specific gravity is from 4,300 to 4,338.

It effervesces with acids, and melts under the blow-pipe, though not very readily. Placed in a covered crucible, in a hot parlour fire, it lost its transparency.

After exposure to a moderate heat in a melting furnace, it adhered to the crucible, and exhibited signs of fusion; but was not diminished in weight, did not feel caustic when applied to the tongue, nor had it lost its property of effervescing with acids.

Hence

Hence it is probable, that its loss of transparency was rather occasioned by numerous small cracks, than by any escape of the water of crystallization, or of its aerial acid.

EXPERIMENTS.

A. 500 grains, dissolved in muriatic acid, in such a manner that nothing but elastic fluid could escape, lost in solution 104 grains, and there remained an insoluble residuum of nearly 3 grains.

2. In another experiment 100 grains lost in solution 21 grains, and there remained 0,6 of a grain of insoluble matter.

B. 100 grains dissolving in dilute muriatic acid, gave out 25 ounce measures of air. This air was received in quicksilver, and when the spar was wholly dissolved, the solution was boiled, in order to drive out what air might be lodged in it.

2. This air was heavier than atmospheric air; it was readily absorbed by agitation in water, it precipitated lime from lime-water, and it extinguished flame. The water which had absorbed it changed the blue colour of litmus slowly * to a red; so that this elastic fluid was undoubtedly fixed air.

C. The solution (B) by the addition of mild fossil fixed alkali, afforded a precipitate which, after proper washing and drying, weighed 100 grains.

2. This precipitate, upon being again dissolved in marine acid, yielded only 20 ounce measures of fixed air.

* Other acids turn the blue of litmus instantly to a red, whilst water, impregnated with fixed air, does not change the litmus immediately; but, after some seconds, the red colour begins to appear, and then gradually grows more distinct

D. To

D. To a saturated solution in marine acid mild fixed vegetable alkaly was added ; the earth was precipitated, and a quantity of fixed air escaped.

2. The same thing happened when mild fossil alkaly was added.

3. When caustic vegetable alkaly was used, the precipitation took place, but without any appearance of effervescence.

4. 50 parts dissolved in marine acid lost, during the solution, nearly 10,5. This solution, upon the addition of caustic vegetable alkaly, let fall a precipitate which, when washed and dried, weighed 45,5.

5. Phlogificated alkaly precipitated the whole of the earth from part of the solution D ; for mild fixed alkaly afterwards added to the filtered liquor occasioned no further precipitation.

E. Part of the precipitates D. 1. 2. after exposure to a strong heat in a crucible, was thrown into water. Next morning the water was completely covered with an ice-like crust, and had the acrid taste of lime-water in a very high degree.

2. The smallest portion of vitriolic acid added to this water occasioned an immediate and copious precipitation ; and when this acrid water was diluted with 200 times its bulk of pure water, the precipitation upon the addition of vitriolic acid was yet sufficiently obvious.

3. A single drop of this acrid water, added to solutions of tartar of vitriol, GLAUBER'S salt, vitriolic ammoniac, alum, Epsom salt, selenite, occasioned an immediate precipitation in all of them.

F. The precipitate thrown down by the caustic vegetable alkaly (D. 3.) was put into water, in expectation that it would make lime-water ; but neither upon standing, nor after boiling, did this water exhibit any precipitation when concentrated
vitriolic

vitriolic acid was dropped in it; nor had it any acrimonious or other peculiar taste.

G. Concentrated vitriolic acid was added to one portion of the precipitate D. 3.; concentrated nitrous acid to a second portion; and marine acid to a third portion. No effervescence could be observed, nor was there any appearance of solution. After standing one hour water was added; and the acids, thus diluted, were suffered to remain upon the portions of the precipitate for another hour. They were then decanted, and saturated with mild fossil fixed alkaly, but without any appearance of precipitation.

H. The part precipitated by the phlogisticated alkaly, when mixed with nitre and borax, and fluxed by a blow-pipe upon charcoal, formed a black glass; upon flint-glass, a white; and upon a tobacco-pipe an opaque yellowish white one.

2. Another portion melted with soap and borax in a crucible, formed a black glass, but without any metallic appearance.

I. The insoluble residuum (A.) was boiled in water, the water decanted, and mild fixed alkaly added, but without any subsequent precipitation.

2. This insoluble powder was not attacked by the nitrous or marine acids; but being put into vitriolic acid, and boiled a considerable time until the acid became highly concentrated, it dissolved entirely, and separated again upon the addition of water. It will appear in the sequel, that the same thing happens to marmor metallicum, when dissolved by boiling in the acid of vitriol.

CONCLUSIONS.

Hence it appears, that 100 parts of this spar contain

Terra ponderosa pura	-	78,6
Marmor metallicum	-	,6
Fixed air	-	20,8
		<hr/>
		100
		<hr/>

OBSERVATIONS.

1st, The quantity of mild fixed alkaly necessary to saturate an acid, previously united to the terra ponderosa, contains more fixed air than is necessary to saturate that quantity of terra ponderosa D. 1. 2.

2dly, The terra ponderosa, when precipitated from an acid by means of a mild fixed alkaly (D. 1. 2.), readily burns to lime; and this lime-water proves a very nice test of the presence of vitriolic acid. E. 2. 3.

3dly, It is very remarkable, that the terra ponderosa spar, in its native state, will not burn to lime. In the lower degrees of heat it suffers no change, as was before observed, besides the loss of its transparency. When urged with a stronger fire, it melts and unites to the crucible, but does not become caustic.

I buried it in charcoal-dust in a covered crucible, and then exposed it to a pretty strong heat; but it did not part with its air.

May we not conjecture, then, that as caustic lime cannot unite to fixed air without the intervention of moisture, and as this spar seems to contain no water in its composition, that it is

is the want of water which prevents the fixed air assuming its elastic aërial state? This supposition becomes still more probable, if we observe that when the solution of the spar in an acid is precipitated by a mild alkaly, C. 1. 2. some water enters into the composition of the precipitate, for it weighs the same as before it was dissolved, and yet contains only 20 ounce measures of fixed air, whilst the native spar contained 25 ounce measures; so that there is an addition of weight equal to that of 5 ounce measures of fixable air, or $3\frac{1}{4}$ grains to be accounted for, which can only arise from the water; and this precipitate, thus united to water, readily loses its aërial acid in the fire, E. 1.

4thly, Professor BERGMAN supposes the terra ponderosa to be a metallic earth*; its entire separation from an acid by means of the phlogisticated alkaly (D. 5.) certainly favours such a supposition; but, if it be so, it is evident from experiments H. 1. 2. that other means than those commonly employed must be used to effect its reduction.

5thly, The precipitate made by the caustic vegetable alkaly D. 4. taking some of the alkaly down with it, and thus forming a substance neither soluble in water nor in acids, is a very curious phænomenon.

I afterwards varied the experiment by adding the terra ponderosa lime-water (E.) to caustic vegetable and caustic fossil alkaly. In both cases this insoluble compound was immediately formed; but not so when caustic volatile alkaly was used. This composition of an alkaly and an earth certainly deserves more attention than I am at present able to bestow upon it.

6thly, As it appears from experiments D. 1. 2. 3. 4. that

* See preface to his *Sciagraphia Regni Mineralis*.

fixed alkalies, both mild and caustic, separated the terra ponderosa from marine acid, I was at a loss to know why Professor BERGMAN, in his admirable table of simple elective attractions, placed the terra ponderosa caustica immediately under the vitriolic, nitrous, and marine acids, and consequently above the caustic alkalies. I was interested in the reality of the facts, because I had so seldom seen reason to doubt the observations of that very excellent chemist, and therefore made the following experiments.

To different portions of terra ponderosa salita and terra ponderosa nitrata I added, drop by drop, caustic vegetable, caustic fossil, and caustic volatile alkalies. In every case the EARTH was thrown down; and I have so often repeated these experiments to satisfy myself and others, that I am persuaded the terra ponderosa caustica ought to be placed below the alkalies, except in the column appropriated to the vitriolic acid; and it may be separated even from that acid, by the vegetable fixed alkali, if the alkali be applied *via sicca*, as will appear in the next section.

7thly, The necessity for using pure acids upon many occasions, and the difficulty of obtaining them pure, are sufficiently obvious. The VITRIOLIC ACID, as made in the large way, is generally pure enough for most purposes. It is apt to get coloured by inflammable matter; but this is seldom an inconvenience; and, when it would be so, it is easy to drive it off by boiling the acid in a Florence flask over a common fire. But there is another cause of impurity in this acid, which appears upon diluting it with water; for then it becomes milky, and in a short time a powder subsides.*

The

* About two years ago I examined this powdery matter; both that which was thrown

The acid may be freed from this powder either by distillation in glass vessels, which is a tedious and dangerous process, or by the affusion of water; and, after the powder has subsided, a gentle evaporation will drive off most of the superfluous water.

NITROUS ACID may be freed from vitriolic and marine acids, by solution of silver in the acid of nitre, as is daily practised; but the MARINE ACID has not, to my knowledge, been purified by any other method than the laborious one of re-distilling it from common salt. It is generally mixed with vitriolic acid, and often in large proportion. There is no temptation, and scarcely an opportunity, for it to be contaminated by nitrous acid. From the vitriolic acid then it may be readily purified by the addition of terra ponderosa caustica dissolved in water, or by the terra ponderosa salita. If the latter be used, a small

thrown down by dilution with water, and also some which Dr. PRIESTLEY gave me, being the residuum of vitriolic acid distilled to dryness in a flint-glass retort.

1st, Repeated boiling in water, reduced $6\frac{1}{2}$ grains to 2 grains.

2dly, This solution, by gentle evaporation, afforded 5 grains of crystals, as hard and as tasteless as selenite.

3dly, To these crystals, re-dissolved in water, mild fossil alkali was added, and a white powder precipitated.

4thly, This powder, after exposure to a pretty sharp heat, was thrown into water; part of it dissolved, and the water got the taste and other properties of lime-water.

5thly, The insoluble part (1.) suffered no change by boiling in nitrous acid; one-half of it mixed with borax, and exposed to the blow-pipe upon charcoal, vitrified; the other half, mixed with borax and charcoal-dust, likewise vitrified.

CONCLUSIONS. It appears, then, that the greater part of this substance was calx vitriolata, or selenite; the remainder a vitrifiable earth.

I had before found, that the terra ponderosa vitriolata, or heavy gypsum, would dissolve in concentrated vitriolic acid; but always separated in a powdery form upon the affusion of water; and now it appears, that calx vitriolata, or selenite, does the same.

portion

portion of the acid must first be tried in a diluted state, from whence we must judge how much of the terra ponderosa salita the whole will require; or else the whole of the acid must be diluted with water. Whether we use the terra ponderosa dissolved in water or in marine acid; in either case the acid of vitriol immediately seizes upon it, and subsides with it in form of an insoluble powder.

As there are reasons for preferring the marine acid in several of the nicer and more important enquiries of chemistry, this ready method of purifying it cannot but prove acceptable.

S E C T I O N II.

Terra ponderosa vitriolata. BERGMAN'S Sciagraphia, §§ 58. 89.

Variety, Heavy Gypsum. Ponderous Spar.

Marmor Metallicum. CRONSTEDT Min. § 18. 2. 19. C.

From Kilpatrick-hills near Glasgow. A sort, with smaller crystals, amongst the iron ore about Ketley in Shropshire. In the lead mines at Alston-Moor.

More obvious Properties.

White; nearly transparent, but has not the property of double refraction; composed of laminæ of rhomboidal crystals; decrepitates in the fire. Specific gravity from 4,402 to 4,440.

E X P E-

E X P E R I M E N T S.

A. 100 grains exposed to a red heat for one hour, in a black lead crucible, lost five grains in weight; but as a sulphureous smell was perceptible, I suspected that a decomposition had taken place, and therefore exposed another portion to a similar heat for the same space of time in a tobacco-pipe. This had no smell of sulphur, nor was it diminished in weight.

2. It is barely fusible under the blow-pipe; but with borax fluxes readily into a white opaque glass.

B. 100 grains, ground in a mortar, and washed over extremely fine by repeated additions of water, were boiled in the same water, and, after setting, the water was poured off. The powder, when dried, had not sensibly lost weight.

2. To separate portions of the washing water, were added mild vegetable and mild fossil alkaly; but without any appearance of precipitation. Nitre of mercury gave a very slight brownish cloud, barely discernible; and nitre of silver an extremely slight bluish appearance.

3. The same powder, boiled again in fresh water, did not affect the water at all; for it stood the test of nitre of silver without any change.

C. Portions of the powder B. were boiled in vitriolic, nitrous, and muriatic acids, of the usual strength, for several minutes. The acids were then saturated with vegetable fixed alkaly, but without any appearance of precipitation, nor had the portions of powder lost any weight.

2. But when boiled in vitriolic acid, until that acid became very much concentrated and nearly red-hot, the whole of it dissolved; but, separated again upon the addition of water, was

not altered in its weight, was not acted upon by acids of the usual strength, and had, under the blow-pipe, the properties mentioned at A. 2.

3. Some of the solution in the concentrated vitriolic acid was left exposed to the atmosphere, that the acid might slowly attract water. After some days, beautiful crystals appeared in the shapes of stars, fasciæ, and other radiated forms.

4. To another portion of this solution mild fixed vegetable alkali was added; but the precipitate appeared to be the marmor metallicum unchanged.

D. One ounce of this marmor metallicum in fine powder was fluxed in a crucible with two ounces of salt of tartar, until it ran thin. This substance, boiled with water in a Florence flask, left a residuum of six drams.

E. This residuum was thrown into water, and pure nitrous acid added, until there was no more effervescence. The undissolved part weighed 52 grains.

F. This undissolved part appeared to be the original substance no ways changed; for it did not dissolve in nitrous or marine acids, but did wholly dissolve in the greatly concentrated and boiling vitriolic acid, from which it was again separated by the addition of water. (C. 2.)

G. The solution D. was saturated with distilled vinegar, and then evaporated to dryness, but with less than a boiling heat. The sal diureticus, thus formed, was washed away with alcohol. The remaining salt weighed 5 drams nearly.

2. This salt had the appearance and the taste of vitriolated tartar; it decrepitated in the fire; roasted with charcoal-dust, it formed a hepar sulphuris; and with muria calcarea gave a precipitation of selenite.

H. The salt, formed with the nitrous acid (E), shot readily into beautiful permanent crystals, of a rough bitterish taste.

2. Some of this salt was deflagrated with nitre and charcoal, and the alkaly afterwards washed away.

3. The residuum, being the earth of the marmor metallicum, was very white, burnt to lime, and again formed an insoluble compound with acid of vitriol,

I. 100 grains of terra ponderosa aërata were dissolved in diluted marine acid. Vitriolic acid was dropped into this solution, until no more precipitation ensued. The precipitate, after very careful washing and drying, was exposed to a red heat in a covered tobacco-pipe for half an hour; when cool, it weighed 117 grains.

2. 50 grains of terra ponderosa aërata in a lump were put into diluted vitriolic acid; but the action of the acid upon it was hardly sensible, even when made hot.

Marine acid was then added, and after some time an effervescence appeared. The terra ponderosa vitriolata, thus formed, after proper washing and drying, was exposed to a red heat for an hour: it then weighed 58.4 grains.

CONCLUSIONS.

1st, It appears that the marmor metallicum is composed of vitriolic acid and terra ponderosa, D. E. F. G. H.

2dly, That this compound, though probably soluble in water, has so little solubility as almost to escape detection by the nicest chemical tests, B. 1. 2. 3.

3dly, That it is not soluble in acids of the usual strength; but that it perfectly and entirely dissolves in highly concen-

trated vitriolic acid, from which it again separates entire and unchanged upon the affusion of water, C. 1. 2.

4thly, That it cannot be decomposed (*via humida*) by mild fixed alkaly, C. 4.

5thly, That it may be decomposed (*via secca*) by the vegetable fixed alkaly, D. E. G. H.

6thly, That it may be decomposed by inflammable matter, uniting to its acid, and forming sulphur; but that it cannot be decomposed by heat alone, A. 1.

7thly, From experiments I. 1. 2. it appears, that 100 parts of this substance contain

Vitriolic acid pure	32,8
Terra ponderosa pure	67,2
	<hr/>
	100

For the 100 parts of terra ponderosa aërata made use of in the experiment I. 1. would lose during the solution 20,8 of fixed air (§ 1st, A.); then, deducting 0,6 for the marmor metallicum contained in the terra ponderosa aërata (§ 1st. A. 1. 2.), there remains 78,6 of pure terra ponderosa. This, when saturated with vitriolic acid, and made perfectly dry, weighed 117; consequently it had taken 38,4 of vitriolic acid.

OBSERVATIONS.

The apparent insolubility of terra ponderosa aërata in the diluted vitriolic acid (I. 2.) can be accounted for by remarking, that the moment the surface of the lump was acted upon by the acid, an insoluble coat of marmor metallicum was formed upon it, which effectually precluded any further operation of the acid.

Professor

Professor BERGMAN, in order to obtain the earth from the terra ponderosa vitriolata, directs the latter to be roasted with fixed alkaly, and the dust of charcoal; but I have always done it by charcoal dust alone, though probably this method may require a greater degree of heat.

It has been remarked, that terra ponderosa and calx of lead resemble each other in many respects; and I must add, that the vitriol of lead dissolves in the highly concentrated vitriolic acid much in the same manner that the marmor metallicum does, and like this too separates upon the affusion of water; but I never observed it disposed to crystallize.

The marmor metallicum may probably be useful in some cases where a powerful flux is wanted; for I once mixed some of it with the black flux, and exposing it to a pretty sharp heat, it entirely ran through the crucible. May not, therefore, some of the more common varieties of it be used advantageously as a flux to some of the more refractory metallic ores?

S E C T I O N III.

Terra ponderosa Vitriolata.

Variety, Calk or Cauk.

Marmor Metallicum, CRONSTEDT Min. § 18. B?

Plentiful in the Mines in Derbyshire.

More obvious Properties.

Of a white or reddish colour; crystallized in rhomboidal laminæ, but these very much intermixed and confused. Loses

S f 2

little

little or nothing of its weight by being made red-hot. Specific gravity 4,350.

EXPERIMENTS.

A. Ground in a mortar, and washed over, the washing water, when decanted, gave no precipitation with mild vegetable alkaly; but with nitre of silver and nitre of mercury the slightest cloud imaginable.

B. 100 grains boiled in marine acid weighed, after proper washing and drying, 99,5.

C. The acid solution B let fall a Prussian blue upon the addition of a single drop of phlogificated fixed alkaly; and, when saturated with mild fossil alkaly, afforded an ochry-coloured precipitate.

D. This precipitate, collected and washed, weighed half a grain. It was roasted with tallow, and then was wholly attracted by a magnet.

E. A quantity of the cauk, finely powdered, was mixed with charcoal-dust, and roasted in a crucible at a white heat, for five hours, fresh charcoal-dust being occasionally added. It gave out a strong smell of sulphur.

F. To this roasted cauk nitrous acid was added, which dissolved the greater part of it; producing, during the solution, some effervescence, and a strong smell of hepar sulphuris.

G. Some of this solution, after proper evaporation, afforded beautiful crystals, not deliquescent, exactly resembling those obtained from the marmor metallicum, (§ II. H.).

H. To other portions of the solution F, were added fixed vegetable and fossil alkalies, and likewise volatile alkaly, each of which precipitated the earth from the acid.

I. This

F. This earth, after exposure to a white heat for one hour, became caustic, and made lime-water, similar in properties to that mentioned at § 1st. E.

K. Some of the part not acted upon by the nitrous acid F, dissolved entirely by boiling in highly concentrated vitriolic acid, and wholly separated again by the affusion of water. More water was added, and the whole was boiled again; but the filtered liquor gave no signs of precipitation upon the most liberal addition of mild fixed vegetable alkaly.

CONCLUSIONS.

It appears, therefore, that 100 parts of Derbyshire cauk contain.

Marmor metallicum.	-	99,5
Calciform iron.	-	,5
		<hr/>
		100

And it is probable, that the redder pieces contain a little more iron.

SECTION IV.

Terra ponderosa vitriolata.

Variety, radiated Cauck.

Gypsum crystallifatum capillare. CRONSTEDT Min.

§ 19. B.

From Pennely by the Bog, near Minsterley, in Shropshire, fifteen miles from Salop, on the road to Montgomery.

Mere

More obvious Properties.

Somewhat glossy like satin; yellowish-white, opaque; composed of slender spiculæ set close together, and pointing from a center.

In some pieces there are concentric circles of a semi-transparent horn like appearance. It is not very brittle; may be shaved with a knife; loses little or nothing of its weight by being made red-hot. Its specific gravity 4,000; but after soaking one night in the water 4,200, or more.

E X P E R I M E N T S.

When treated in the same manner that the Derbyshire cauk was, in the preceding section, 100 parts of it appeared to contain

Marmor metallicum	-	97,7
Calciform iron	-	2,3
		<hr/>
		100

Suspecting that the presence of so small a proportion of iron could hardly occasion the whole of the apparent differences betwixt the Shropshire and Derbyshire cauks and the marmor metallicum; and thinking it not improbable, that they might contain lead; I mixed some of them with charcoal-dust and borax, but could not by means of the blow-pipe produce any metallic appearance, although vitriol of lead, treated in the same manner, was readily reduced.

I then mixed four parts of cauk with one part of vitriol of lead; the lead could still be reduced, though not so readily as before.

G E N E R A L

GENERAL OBSERVATIONS.

The terra ponderosa seems to claim a place betwixt the earths and the metallic calces. Like the former, it cannot be made to assume a metallic form; but, like the latter, it may be precipitated from an acid, by means of phlogisticated alkaly. In many of its properties it much resembles the calx of lead; and in others, the common calcareous earth, but still seems sufficiently different from that to constitute a new genus, as will appear from a little attention to the following circumstances.

Terra ponderosa,	Terra calcarea,
When dissolved in water, precipitates upon the addition of the smallest portion of vitriolic acid.	Dissolved in water, does not precipitate upon the addition of vitriolic acid.
Its gypsum, therefore, is insoluble.	Its gypsum, therefore, is soluble.
With the nitrous and marine acid, forms crystals which do not deliquesce..	With nitrous and marine acids forms salts so deliquescent that they cannot be kept in a crystallized form..
Decomposes vitriolic salts <i>via humida</i> .	Does not decompose vitriolic salts.

It has been called terra ponderosa, or heavy earth, upon account of the great specific gravity of its gypsum; its spar is likewise heavy enough to countenance such an appellation; but the earth itself does not appear to be a heavy substance, and I imagine the great weight of its compounds with the vitriolic and aërial acids is owing to the absence of water.

Birmingham, Nov. 1783.



XXIV. *Observations du Passage de Mercure sur le Disque du Soleil le 12 Novembre, 1782, faites à l'Observatoire Royal de Paris, avec des réflexions sur un effet qui se fait sentir dans ces mêmes Observations semblable à celui d'une Réfraction dans l'Atmosphère de Mercure. Par Johann Wilhelm Wallot, Membre de l'Académie Electorale de Sciences et Belles Lettres de Manheim, &c. Communicated by Joseph Planta, Esq. Sec. R. S.*

Read April 29, 1784.

1. **L**ES passages de Mercure sur le disque du soleil sont d'autant plus intéressans pour les astronomes, qu'ils donnent principalement le moyen de déterminer avec plus d'exactitude la position des nœuds de son orbite, et que la difficulté de voir cette planète dans ses autres aspects avec le soleil en rend les observations plus précieuses.

2. Deux circonstances assez défavantageuses qui devaient accompagner particulièrement le passage dont il s'agit ici, savoir la proximité du soleil de l'horizon, et Mercure passant trop près du bord de cet astre, semblaient par leur nature offrir trop d'inconvéniens pour en espérer des observations bien exactes; cependant l'encouragement qu'a donné le beau tems qu'il fit toute la journée du 12 Novembre, nous ayant fait apporter une plus grande attention aux observations, nous autorise maintenant à en avoir une meilleure opinion. Je crois pouvoir assurer sans ostentation d'y avoir réussi assez pour être satisfait des miennes, et pour oser les garantir autant que la nature des choses

chofes peut le permettre. Si je puis me flatter d'avoir obtenu de ce passage une observation très exacte et peut-être la plus complète, je ne dissimulerai pas que je dois en grande partie cet avantage à M. DE CASSINI qui, m'ayant laissé la meilleure lunette * qu'il y ait à l'Observatoire Royal, m'avait mis par là dans le cas d'employer la plus grande vigilance pour mériter par l'exactitude de mes opérations la confiance qu'on me témoignait dans une occasion aussi importante.

3. Nous avons fait (M. DE CASSINI et moi) toutes les observations nécessaires pour constater avec la plus grande exactitude l'état de notre pendule; et, en réduisant mes observations au tems vrai, je n'ai pas même négligé les dixièmes de seconde. Cette précision scrupuleuse paraîtra peut-être superflue dans de pareilles observations, mais on verra par la suite de ce Mémoire les raisons qui m'y ont déterminé. Voici mes observations dans le même ordre où elles se sont faites, et réduites au tems vrai de la méridienne de l'Observatoire Royal de Paris.

Tems vrai.		
h. "		
Entrée	à 2 56 28,8	Je soupçonne la planète. Contact extérieur de l'entrée.
	à 2 58 28,8	J'estime Mercure entré à moitié. Centre de \odot sur le bord du \odot .
	à 3 2 3,8	Contact intérieur de l'entrée.
	à 3 3 45,8	Mercure absolument détaché du soleil.

En mesurant le diamètre de Mercure sur le disque du soleil je l'ai trouvé par deux fois exactement de la même quantité, savoir de 9 parties du micromètre objectif, qui valent $9'',535$ de degrés du grand cercle.

Sortie	à 4 17 18,4	Contact intérieur de la sortie.
	à 4 20 36,4	Le centre de Mercure sur le bord du soleil.
	à 4 22 53,4	Contact extérieur de la sortie. Mercure totalement perdu de vue.

* Une excellente lunette achromatique de DOZLOND de 3 pieds.

Le bord du soleil était si ondoyant que Mercure, aux approches de sa sortie totale, ressemblait exactement à un corps flottant sur les vagues d'une eau fortement agitée, et qui tantôt disparaît entièrement, tantôt élevé par les vagues se montre en partie et quelquefois tout entier. Ces vagues ou ondulations allaient toujours dans le même sens du N. Ouest au Sud Est. Leur mouvement était assez rapide, et c'est précisément la rapidité de ce mouvement qui m'a singulièrement favorisé l'observation du contact extérieur de la sortie de Mercure, parceque je ne la perdais jamais de vue qu'un instant.

Je terminerai le détail de mes observations par assurer, que je n'ai pas apperçu la moindre apparence d'une atmosphère ou nébulosité autour de Mercure pendant toute la durée de son passage, quoique la lunette me représentât tous les objets très distinctement. J'ai toujours vu le disque de Mercure bien noir, et également bien terminé dans toute la circonférence qui me paraissait toujours tranchée nette, surtout dans le commencement où les ondulations étaient moins fortes, jusques vers le milieu du passage. Mais cela ne m'empêchera pas d'être très persuadé de l'existence d'une atmosphère autour de Mercure, comme autour de tous les corps célestes, et qu'on peut fort bien l'avoir apperçue dans ce passage sous un ciel plus pur et plus beau que celui de Paris.

Résultats du calcul des observations précédentes selon leurs différentes combinaisons.

4. La méthode que j'ai suivie pour réduire les observations de ce passage au centre de la terre, m'est en quelque sorte particulière; mais comme elle n'est pas entièrement nouvelle puisqu'elle

qu'elle ne diffère de toutes les méthodes connues qu'en ce que je l'ai simplifiée en la rendant absolument directe, je me contenterai d'en donner une idée générale. Je n'ai employé dans mes calculs que ce qui est donné directement par observation, ou bien des quantités plus exactement données par les tables, telles que le diamètre du soleil, son mouvement horaire et celui de Mercure. Mais ce qui caractérise essentiellement cette méthode, c'est qu'en combinant les observations toujours ensemble deux à deux, on a la durée ou le tems écoulé d'une observation à l'autre qui est une des principales données du problème, et la plus exacte qu'on puisse se procurer par observation. Or, quand l'observation nous fournit directement des données exactes, je ne vois absolument pas la nécessité d'en aller chercher de moins exactes pour les faire entrer dans le calcul. C'est pourtant ce que font quelques astronomes modernes*, qui, en recommandant dans leur Traité d'Astronomie de calculer les observations séparément afin, disent-ils, de multiplier les résultats et d'en déduire plus exactement par un milieu la quantité qu'on cherche, sont obligés pour cet effet de supposer à peu près connu le milieu du passage et la plus courte distance des centres†. Ce raisonnement, aussi éloigné des principes de la géométrie, que des règles de l'analyse, me paraît encore illusoire quant à l'exactitude qu'on espère obtenir de la multiplicité des résultats ainsi déterminés ; voici pourquoi.

5. Je suppose pour un instant qu'on prenne au hasard deux observations, et qu'on les calcule séparément chacun suivant ce précepte ; il est certain que si l'on ne suppose pas le milieu du passage et la plus courte distance des centres tels que les don-

* Principalement M. DE LA LANDE dans son Traité d'Astronomie, édition de 1771, livre XI. art. 2152.

† Ibid. art. 2062 et 2063.

neraient directement ces deux observations combinées ensemble, on doit trouver, pour la quantité qu'on cherche, deux résultats différens, et qui différeront d'autant plus que la supposition qu'on aura faite sera plus éloignée de la véritable. On prend donc alors un milieu entre les deux résultats et l'on s'imagine avoir trouvé la vérité ; mais il me semble qu'il est très permis d'en douter, car, outre qu'il y a bien des cas où l'on ne peut pas regarder le résultat moyen comme le véritable, ici ce n'est pas même admissible, puisque le milieu du passage et la plus courte distance des centres sont deux quantités qui dependent l'une de l'autre, et qu'il est impossible de les supposer telles précisément qu'elles se conviennent relativement à deux observations déterminées, à moins que ce ne soit un effet du hazard. Or si je suppose maintenant qu'on prenne les deux mêmes observations, et qu'on les combine ensemble, il est clair qu'on ne trouvera qu'un seul résultat pour la quantité cherchée, mais ce sera précisément la même qu'on aurait eue par un milieu entre les deux résultats trouvés suivant l'autre manière si l'on y avait fait une supposition qui s'écartât peu de celle qu'il convenait de faire. Il s'en suit donc qu'on serait arrivé au même but par les deux méthodes, mais avec cette différence que les quantités déterminées d'après la méthode des combinaisons sont dans tous les cas de vrais résultats tels que les donne véritablement l'observation, tandis que d'après l'autre ce ne sont que des résultats fictifs ou approchés. Le calcul dévient à la vérité plus long, lorsqu'il y a plus de trois observations ; parceque alors le nombre des combinaisons qu'on en peut faire deux à deux, conséquemment aussi le nombre des résultats qui en proviennent, surpassera toujours celui des observations. Or si pour déterminer une quantité quelconque d'après une méthode on risque de trouver des résultats inexacts, et que d'après une autre méthode

thode on peut déterminer la même quantité sans courir et danger, il est incontestable que celle-ci est préférable à l'autre. Lorsqu'on ne peut avoir que des observations isolées, il faut bien alors se résoudre à les calculer séparément, mais encore avec la restriction que les quantités qu'on supposera connues soient données par d'autres observations, qui étant dans le cas d'être combinées deux à deux, soient elles-mêmes très exactes. Il est donc aisé de conclure de tout ce que je viens de dire que la manière de calculer séparément chaque observation, non seulement ne procure pas les avantages qu'on en attend pour la multiplicité des résultats, mais elle est encore moins exacte que celle de combiner deux à deux les observations, ainsi que l'enseignent les plus célèbres astronomes. Je ne me suis permis d'entrer dans ces détails que pour prouver à la Société Royale que je ne me fers jamais avec confiance d'aucune méthode sans l'avoir examinée auparavant en la créant, pour ainsi dire, une seconde fois.

6. J'ai calculé le lieu du soleil et de Mercure par les tables de HALLÉY pour 2½ h. 3½ h. et 4½ h. espace de tems qui comprend à peu près par son milieu toute la durée du passage, et j'ai trouvé,

	à 2 h 30' tems vrai	à 3 h. 30' t. v.	à 4 h. 30' t. v.
	S. ° ' "	S. h. ' "	S. ° ' "
La longitude du soleil de	7 20 22 43,6	7 20 25 14,8	7 20 27 45,9
Son ascension droite	7 17 55 55,3	7 17 58 28,4	7 18 1 1,5
Sa déclinaison - - australe	17 51 49,6	17 52 29,9	17 53 10,1
La longit. géocentrique de Mercure	7 20 32 2,9	7 20 28 40,8	7 20 25 18,4
Sa latitude - - boréale	0 14 31,0	0 15 22,6	0 16 13,8
Ce qui me donne		entre 2½ h. et 3½ h.	entre 3½ h. et 4½ h.
		° ' "	° ' "
Le mouvem. horaire relatif Merc. sur l'écliptique de		5 33, 3	5 53, 5
L'inclinaison de l'orbite relative sur l'écliptique de		8 18 33, 8	8 14 28, 5
Le mouvem. horaire relatif de Merc. dans son orbite		5 57,05	5 57,19

Je

Je me suis servi de l'inclinaison et du mouvement horaire qui avait lieu entre $2\frac{1}{4}$ h. et $3\frac{1}{4}$ h. dans le calcul des observations du commencement, et l'inclinaison avec le mouvement horaire qui avait lieu entre $3\frac{1}{4}$ h. et $4\frac{1}{4}$ h. m'a servi pour la fin du passage. Quant aux autres élémens, j'ai employé le diamètre du soleil de $32' 24'',5$; celui de Mercure de $9'',535$ comme je l'ai mesuré sur le disque du soleil pendant le passage, et la parallaxe horizontale du soleil dans ses moyennes distances de $8',7$ telle que je l'ai établie dans mon Mémoire sur le passage de Venus en 1769. D'où j'ai conclu la différence des parallaxes horizontales du soleil et de Mercure pour le jour du passage 12 Novembre de $4'',088$.

7. Avec ces éléments j'ai calculé les observations des contacts en ne négligeant pas même les millièmes de seconde dans certains cas; je n'ai mis cette scrupuleuse exactitude dans tous mes calculs que parceque je voulais m'assurer dans le cas où je viendrais à trouver des différences entre les résultats de même dénomination que je n'eusse à les attribuer uniquement qu'aux observations. La table suivante renferme les résultats les plus importants de ces calculs.

Table

Table des résultats du calcul des observations des contacts et du centre de Mercure.

	Contacts intérieurs	Contacts extér.	Centre de γ
	h. ' "	h. ' "	h. ' "
Heure vraie de l'observation { entrée fortie	3 2 3, 8 4 17 18, 4	2 56 28, 8 4 22 53, 4	2 58 28,8 4 20 36,4
Durée donnée directement par l'obs.	1 15 14, 6	1 26 24, 6	1 22 7,6
Plus courte distance des centres vue à la surface de la terre	15 41, 2	15 42, 5	15 41,0
Heure vraie du milieu du passage pour le centre de la terre	3 39 47, 4	3 39 47, 1	3 39 38,7
Plus courte distance des centres vue du centre de la terre	15 45, 1	15 46, 4	15 44,9
Réduction de l'observation { entrée au centre de la terre fortie	+ 2 59,45 - 2 46,70	+ 2 34,38 - 2 22,27	+ 2 42,9 - 2 30,7
Heure vr. de l'observation { entrée arrivée pour le centre γ fortie	3 5 3,25 4 14 31,70	2 59 3,11 4 20 31,13	3 1 11,7 4 18 5,7
Durée du passage pour le centre de la terre	1 9 28,45	1 21 28,02	1 16 54,0
Heure vraie de la conjonction de Mercure et du soleil	4 2 53, 2	4 2 54, 8	4 2 44,1
Latitude de γ en conjonction donnée par observation	S. ° ' " 15 55,1	S. ° ' " 15 56,4	S. ° ' " 15 54,8
Longitude du soleil ou de Mercure en conjonction	7 20 26 37,6	7 20 26 37,7	7 20 26 37,2
Longitude de Mercure en conjonction donnée par les tables eu égard à l'aberration	7 20 27 8,4	7 20 27 8,3	7 20 27 8,9
Latitude de γ en δ donnée par les tables . . . boréale	15 50,7	15 50,7	15 50,5
Erreur des tables { en longitude en latitude	- 30,8 + 4,4	- 30,6 + 5,7	- 31,7 + 4,4
En adoptant la latitude de γ au moment de la δ donnée par les contacts intérieurs de 15' 55", je trouve le { S. ° ' " 15 45 22,8 en supposant l'inclinaison de l'orbite 7 ° 0 0 avec M. CASSINI. lieu du Ω { 15 44 55,7 en supposant l'inclinaison de l'orbite 6 59 20 avec HALLEY. de γ à			

23

8. L'on

8. L'on voit par cette table que les contacts intérieurs donnent l'heure du milieu du passage à 3 dixièmes de seconde près la même que les contacts extérieurs; l'heure de la conjonction à $1''{,}6$ près la même*, et la plus courte distance des centres ainsi que la latitude de Mercure en conjonction de $1''{,}3$ plus petite. Quant aux deux observations du centre de Mercure sur les bords du soleil, elles donnent le milieu du passage de $8''{,}7$ plutôt que les contacts intérieurs, et la plus courte distance des centres ainsi que la latitude de Mercure en conjonction de 2 dixièmes de seconde seulement plus petite. Cette différence dans l'heure du milieu du passage ne peut venir que de la manière dont j'ai estimé le centre de Mercure; car il y a d'abord seconde pour seconde le même intervalle de tems entre les deux contacts de l'entrée qu'entre ceux de la sortie, c'est à dire l'un et l'autre de $5' 35''$. Ensuite je trouve qu'il s'était écoulé $2' 0''$ depuis le contact extérieur de l'entrée jusqu'au moment où j'ai estimé le centre de Mercure sur le bord du soleil, au lieu de $2' 17''$ qu'il y a entre les pareilles observations de la sortie; mais cet intervalle de tems devant être le même pour l'entrée et pour la sortie, la différence $17''$ fait voir que j'ai estimé le centre de Mercure plus près du contact extérieur à l'entrée qu'à la sortie, ce qui devait aussi avancer l'instant du milieu du passage; or la moitié de ces $17''$ fait précisément les $8''\frac{1}{2}$ dont le milieu du passage est arrivé plutôt selon cette observation que selon celle des contacts intérieurs (puisque l'erreur de l'une des deux observations n'est que la moitié sur le milieu du passage). J'ai donc marqué l'instant de l'observation du centre à l'entrée plutôt qu'il

* L'instant de la conjonction diffère de $1''{,}6$ quoique celui du milieu du passage ne diffère que de $0''{,}3$, parceque la portion de l'orbite relative comprise entre le milieu du passage et la conjonction est plus grande pour une plus grande distance des centres.

ne fallait ; car je pencherai toujours à croire plutôt que c'est sur celle de l'entrée que doit tomber l'erreur, parceque n'ayant pas encore vu Mercure sur le disque du soleil, je ne pouvais pas juger de sa grandeur aussi bien qu'à la fin après l'avoir vu pendant toute la durée de son passage. C'est aussi en partie par cette même raison, jointe à celle qu'on ne peut pas estimer avec quelque précision le centre d'un corps qu'on ne voit pas entièrement, que je puis avoir observé le centre de Mercure sur le bord du soleil trop tôt à l'entrée, et trop tard à la sortie relativement aux observations des contacts. Cette discussion, en apparence d'ailleurs peu importante, devient ici d'une grande nécessité, parcequ'il s'agit de montrer les défauts de deux observations que je ne rejette qu'avec beaucoup de regrets ; car l'observation du centre de la planète sur le bord du soleil n'étant pas affectée de l'effet de plusieurs élémens (le diamètre de la planète et l'effet d'une atmosphère qui l'envelopperait) que nous connaissons souvent mal, ou que nous ignorons absolument, offrirait des avantages réels, si elle pouvait se faire avec une certaine précision.

9. Quoique les résultats de mes calculs s'accordent assez pour inspirer quelque confiance, je n'ai cependant pas été trop satisfait de trouver la plus courte distance des centres de $1'',3$ plus grande par les contacts extérieurs que par les contacts intérieurs. Cette différence annonce une erreur dans les durées. Ou la durée du passage entre les deux contacts extérieurs est trop petite, ou celle des contacts intérieurs est trop grande. Mais je me suis imposé la loi de ne jamais faire aucune correction à mes observations lorsque je ne les ai accompagnées d'aucune marque qui me fasse douter de leur bonté ; je ne trouve donc aucune raison qui m'autorise à changer la durée des contacts intérieurs, et quand je voudrais m'écarter ici un moment de mes

principes pour augmenter la durée des contacts extérieurs, je ne le pourrais faire qu'en considération de l'incertitude avec laquelle on peut estimer le contact extérieur de l'entrée trop tard, et celui de la sortie trop tôt, ce qui est toujours probable ; mais je ne la pourrais augmenter que tout au plus de 5 à 6 secondes de tems, puisqu'on a vu dans l'article précédent qu'il n'y a que 17'' d'incertitude sur l'estime des deux observations du centre de Mercure sur les bords du soleil qui comparativement entre elles-mêmes se font beaucoup moins exactement. Or ces 5 ou 6 secondes d'augmentation sur la durée extérieure ne suffisent pas à beaucoup près (car il en faudrait 106'') pour réduire à zéro la différence qui se trouve entre les deux valeurs de la plus courte distance des centres. Il faut donc chercher ailleurs que dans les observations la cause de cette différence. C'est ce que je crois pouvoir trouver dans l'effet d'une atmosphère supposée autour de Mercure, ou d'une cause semblable.

10. D'après les recherches que j'ai faites sur l'atmosphère de Venus à l'occasion de son passage en 1769, et dont j'ai établi et démontré les principes dans un petit *Traité complet sur les passages de Venus et de Mercure*, j'étais prévenu que la circonstance caractéristique de ce passage de Mercure qui était si désavantageuse à l'égard de l'utilité qu'on en retire pour perfectionner les tables, devait être extrêmement favorable à la détermination de l'effet d'une atmosphère qui environnerait Mercure, puisque la planète passant fort près du bord du soleil, son mouvement se faisait très obliquement à ce bord, et agrandissait beaucoup l'effet d'une atmosphère. En conséquence je me suis singulièrement appliqué à observer ce passage et principalement les quatre contacts avec la plus grande attention, afin de me procurer des observations suffisamment exactes pour pouvoir m'en servir avec avantage à déterminer l'effet de cette atmosphère,

sphère, ou du moins à m'assurer de son existence. Je puis dire maintenant que les résultats de mes calculs, de quelque manière que je les combine, en supposant l'observation et le diamètre de Mercure employé dans mes calculs rigoureusement exacts, m'indiquent la présence d'un effet semblable à celui d'une réfraction ou inflexion que souffriraient les rayons solaires dans leur passage auprès du globe de Mercure. Voici comment.

11. J'ai démontré dans le petit Traité que je viens de citer que la combinaison des deux observations des contacts extérieurs doit donner le même instant pour celui du milieu du passage que la combinaison des deux contacts intérieurs, et que cet instant du milieu du passage déduit de l'une et de l'autre combinaison restera toujours absolument le même, qu'on suppose la planète entourée d'une atmosphère ou non. Il est évident qu'à plus forte raison le milieu du passage déduit de la combinaison des deux observations du centre de la planète sur la bord du soleil ne sera point altéré par l'effet d'une atmosphère, puisqu'elle n'influe pas même sur chacune de ces deux observations séparément. Ensuite j'ai encore fait voir que dans la supposition d'une atmosphère autour de la planète qui passe sur le disque du soleil, le milieu du passage déduit de la combinaison de l'observation du contact extérieur de l'entrée avec celle du contact intérieur de la sortie, doit arriver plus tard ; et le milieu du passage donné par la combinaison du contact intérieur de l'entrée avec le contact extérieur de la sortie, doit arriver précisément de la même quantité plutôt que le milieu du passage conclu par la combinaison des deux contacts intérieurs, ou par celle des deux contacts extérieurs, ou, ce qui revient encore au même, que le milieu du passage que donneraient indistinctement toutes les observations des contacts combinées comme on voudra, si la planète n'avait point d'atmosphère. La différence

ou la quantité, dont le milieu du passage est trouvé plus tard ou plutôt, fera l'effet de l'atmosphère de la planète sur le milieu du passage.

12. En conséquence de ces principes j'ai donc fait encore deux combinaisons pour en déduire le milieu du passage, et j'ai trouvé que la combinaison du contact extérieur de l'entrée avec le contact intérieur de la sortie donne cet instant à 3h. 40' 13'',6 ; celle du contact intérieur de l'entrée avec le contact extérieur de la sortie le donne à 3 h. 39' 20'',8. Or on a vu (art. 7.) que le milieu du passage, selon la combinaison des deux contacts intérieurs et celle des deux contacts extérieurs est arrivé à 3 h. 39' 47'',2, quantité qui se trouve entre les deux précédentes et exactement à égales distances de l'une et de l'autre, savoir de 26'',4. Il est donc évident que l'effet de l'atmosphère de Mercure dans ce passage-ci a été 26'',4 de tems sur le milieu du passage, en faisant abstraction de toute autre cause qui peut avoir quelque influence sur les observations des contacts.

13. Mais ces 26'',4 ne peuvent provenir que de trois causes : ou de l'inexactitude des observations, ou d'une erreur sur les diamètres du soleil et de Mercure employés dans les calculs, ou de la réfraction des rayons solaires dans l'atmosphère de Mercure ; ainsi que je l'ai démontré dans mon petit *Traité sur les Passages de Venus et de Mercure*, et où j'arrive, après un examen rigoureux de toutes les hypothèses possibles, à cette équation générale $A = \alpha \pm \beta \pm \gamma + \epsilon$; dans laquelle A est la quantité déterminée par les combinaisons des observations, comme ici les 26'',4, et par conséquent connue ; α , β , γ , ϵ , la part qui en appartient respectivement à l'atmosphère de la planète, à l'erreur de son diamètre, à celle du soleil et à l'erreur de l'observation. Je ferai remarquer seulement au sujet de cette formule qu'il n'y a que l'erreur sur le diamètre de la planète dont l'effet β pourrait quelquefois entrer dans

dans la valeur de A comme quantité négative, mais alors, loin de nuire à l'opinion d'attribuer cet effet, qui est ici de $26'',4$, à l'atmosphère de la planète, elle la favoriserait plutôt. Quant à l'erreur sur le diamètre du soleil, son influence peut être regardée comme nulle dans tous les cas, c'est à dire γ peut toujours être regardé comme zéro, à moins que l'erreur sur le diamètre du soleil ne soit très considérable, et c'est un des avantages de ma méthode pour déterminer la valeur de A . Or la probabilité serait en faveur des observations, puisqu'elles donnent, ce qui est conforme à la théorie, le même intervalle de tems entre les deux contacts de l'entrée qu'entre les deux contacts de la sortie, ainsi l'on aurait ici $\epsilon = 0$. Quant aux deux autres causes, il n'en est pas de même, puisqu'il est évident par la formule générale qu'une même quantité considérée comme erreur sur les diamètres, ou comme réfraction des rayons solaires dans l'atmosphère de Mercure, est capable de produire exactement le même effet. Mais comme il est très probable que les trois causes à la fois peuvent avoir concouru à produire ces $26'',4 = A$, et qu'il est absolument impossible, d'après ma méthode comme d'après toute autre, de démêler les effets pour assigner à chaque cause la part qui lui appartient dans la valeur de A , le problème restera indéterminé à cet égard, par conséquent si l'on ne veut admettre qu'une seule cause, on sera libre de se décider pour l'une ou pour l'autre; or la question n'étant plus alors qu'une affaire d'opinion, le choix doit tomber nécessairement sur la cause qui est la moins connue, et dont nous ne pouvons pas raisonnablement contester l'existence. On peut donc fort bien attribuer cet effet à l'atmosphère de Mercure sans craindre de se tromper beaucoup. Il s'en suit donc qu'en regardant ces $26'',4$ simplement comme effet de l'atmosphère de Mercure, la quantité, qui en résulterait pour l'inflexion ou la réfraction

réfraction réelle de cette atmosphère, nous assurerait au moins d'une espèce de limite qu'elle ne surpasserait jamais ou du moins très-rarement, puisque l'inflexion des rayons solaires, à elle-seule, ne peut égaler la somme des trois causes dont elle fait partie, que dans l'hypothèse particulière des deux autres égales à zéro. Cette manière d'envisager le problème me donnera du moins une connaissance approchée de la valeur de la réfraction de l'atmosphère de Mercure, dont je n'aurais sans cette recherche absolument aucune idée. Or il me semble qu'il vaut mieux acquérir une connaissance imparfaite que de rester dans l'ignorance absolue.

14. La quantité de cet effet, quel qu'il soit, étant donc connue, j'ai cherché à concilier les deux valeurs de la plus courte distance des centres trouvées par les contacts intérieurs et extérieurs, et pour cet effet je me suis proposé ce problème qui doit s'en suivre naturellement, puisque la valeur de γ peut toujours être regardée comme zéro : *Déterminer le diamètre du soleil, celui de Mercure étant connu par observation, tel que la durée donnée par les contacts extérieurs et la durée des contacts intérieurs fassent trouver, l'une et l'autre, la même quantité pour valeur de la plus courte distance des centres.* Ce problème étant résolu en nommant a , la demi-durée entre les contacts extérieurs, b celle des contacts intérieurs, δ le diamètre de Mercure, x la différence des demi-diamètres de Mercure et du soleil, y la plus courte distance des centres cherchée, je trouve $x = \frac{a^2 - b^2 - \delta^2}{2\delta}$ et $y = \sqrt{\left(\frac{a^2 - b^2 - \delta^2}{2\delta}\right)^2 - b^2}$; formules qui étant évaluées après avoir convenablement corrigé des 26,"4 chaque observation des quatre contacts, et augmenté la durée des contacts extérieurs de ces 6" dont j'ai parlé ci-devant, me donnent $x = 967'',04$ valeur plus petite

petite de $0'',44$ que celle que j'avais supposée dans mes calculs, et $y = 15'45'',24$ plus grande de $0'',1$ que celle qui m'a été donnée par la combinaison des deux contacts intérieurs; ainsi tous les résultats trouvés par cette combinaison n'auront besoin d'aucune correction, et je les adopterai, comme étant les meilleurs, tels qu'ils sont rapportés dans la table ci-dessus art. 7.

15. En supposant donc que les $26'',4$ soient produites par l'atmosphère de Mercure, je trouve $0'',276$ pour la réfraction horizontale de cette atmosphère. Les observations du passage de Venus en 1769 m'ayant annoncé un effet semblable d'environ $8''$ à $9''$ de tems, je trouve sa réfraction horizontale d'environ $0'',205$ qui n'est qu'à peu près les deux tiers de celle de Mercure.

CONCLUSION.

16. Quelque peu de confiance que j'attache à ces résultats, et quelque soit l'opinion que j'adopte pour choisir entre les causes qui peuvent produire l'effet en question, je crois du moins pouvoir conclure avec certitude, ce que je m'étais principalement proposé de prouver dans ce Mémoire, que les observations dont il s'agit ici, malgré le degré d'incertitude qu'on puisse leur supposer, indiquent clairement l'existence d'un effet semblable à celui d'une atmosphère qui environnerait la planète; et que cet effet, soit qu'il provienne effectivement de cette atmosphère, ou d'une erreur sur le diamètre de la planète, ou d'une erreur dans les observations, ou qu'il soit le résultat de l'action simultanée des trois causes réunies, il se fait sentir évidemment dans toutes les observations des passages de Venus et de Mercure, du moins dans toutes celles que j'ai calculées. Par conséquent l'influence de ces causes qui altèrent les observations d'une manière si sensible me paraît, sous tous les points de vue, mériter l'attention des astronomes; et je suis très persuadé que faute d'y avoir eu égard

égard dans la comparaison des observations du passage de Venus pour en déduire la parallaxe du soleil, bien des astronomes feraient dans le cas de recommencer leur calcul. Heureusement je n'ai pas ce reproche à me faire ; car j'ai constamment évité avec le plus grand soin l'effet d'une atmosphère autour de Venus en choisissant les observations pour en faire la comparaison de manière que l'effet de cette atmosphère, qu'il ait existé ou non, se trouvait toujours réduit à zéro. C'est ainsi que dans mon Mémoire sur le passage de Venus en 1769 j'ai fixé à $8'',7$ la parallaxe horizontale du soleil dans ses moyennes distances à la terre.

XXV. *Thoughts on the constituent Parts of Water and of Dephlogisticated Air; with an Account of some Experiments on that Subject. In a Letter from Mr. James Watt, Engineer, to Mr. De Luc, F. R. S.*

Read April 29, 1784.

DEAR SIR,

Birmingham,
November 26, 1784.

IN compliance with your desire, I send you an account of the hypothesis I have ventured to form on the probable causes of the production of water from the deflagration of a mixture of dephlogisticated and inflammable airs, in some of our friend Dr. PRIESTLEY's experiments.

I feel much reluctance to lay my thoughts on these subjects before the public in their present indigested state, and without having been able to bring them to the test of such experiments as would confirm or refute them; and should, therefore, have delayed the publication of them until these experiments had been made, if you, Sir, and some other of my philosophical friends, had not thought them as plausible as any other conjectures which have been formed on the subject; and that though they should not be verified by further experiments, or approved of by men of science in general, they may perhaps merit a discussion, and give rise to experiments which may throw light on so important a subject.

I first thought of this way of solving the phenomena in endeavouring to account for an experiment of Dr. PRIEST-

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LEY'S,

LEY's, wherein water appeared to be converted into air; and I communicated my sentiments in a letter addressed to him, dated April 26, 1783*, with a request that he would do me the honour to lay them before the Royal Society; but as, before he had an opportunity of doing me that favour, he found, in the prosecution of his experiments, that the apparent conversion of water into air, by exposing it to heat in porous earthen vessels, was not a real transmutation, but an exchange of the elastic fluid for the liquid, in some manner not yet accounted for; therefore, as my theory was no ways applicable to the explaining these experiments, I thought proper to delay its publication, that I might examine the subject more deliberately, which my other avocations have prevented me from doing to this time.

It has been known for some time, that inflammable air contained much phlogiston; and Dr. PRIESTLEY has found, by some experiments made lately, that it "is either wholly pure phlogiston, or at least that it contains no apparent mixture of any other matter." (In my opinion, however, it contains a small quantity of water and much elementary

* This letter Dr. PRIESTLEY received at London; and, after shewing it to several Members of the Royal Society, he delivered it to Sir JOSEPH BANKS, the President, with a request that it might be read at some of the public meetings of the Society; but before that could be complied with, the author, having heard of Dr. PRIESTLEY's new experiments, begged that the reading might be delayed. The letter, therefore, was reserved until the 22d of April last; when, at the author's request, it was read before the Society. It has been judged unnecessary to print that letter, as the essential parts of it are repeated, almost *verbatim*, in this letter to M. DE LUC; but, to authenticate the date of the author's ideas, the parts of it which are contained in the present letter are marked with double commas.

heat.)

heat *.) “ He found, that by exposing the calces of metals
 “ to the solar rays, concentrated by a lens, in a vessel contain-
 “ ing inflammable air only, the calces of the softer metals
 “ were reduced to their metallic state;” and that the inflam-
 mable air was absorbed in proportion as they became phlogist-
 cated; and, by continually supplying the vessel with inflamma-
 ble air, as it was absorbed, he found, that out of 101 ounce mea-
 sures, which he had put into the vessel, 99 ounce measures were
 absorbed by the calces, and only two ounce measures remained,
 which, upon examination, he found to be nearly of the same
 quality the whole quantity had been of before the experiment,
 and to be still capable of deflagrating in conjunction with at-
 mospheric or with dephlogisticated air. *Therefore, as so great a*
quantity of inflammable air had been absorbed by the metallic calces;
the effect of reducing them to their metallic state had been produced;
and the small remaining portion was still unchanged, at least had
suffered no change which might not be attributed to its original want
of purity; it was reasonable to conclude, that inflammable air must be
the pure phlogiston, or the matter which reduced the calces to
metals.

2. “ The same ingenious philosopher mixed together cer-
 “ tain proportions of pure dry dephlogisticated air and of pure
 “ dry inflammable air in a strong glass vessel, closely shut,
 “ and then set them on fire by means of the electric spark,”
 in the same manner as is done in the inflammable air pistol.
 “ The first effect was the appearance of red heat or inflamma-

* Previous to Dr. PRIESTLEY's making these experiments, M. KIRWAN had proved, by very ingenious deductions from other facts, that inflammable air was, in all probability, the real phlogiston, in an aerial form. These arguments were perfectly convincing to me; but it seems more proper to rest that part of the present hypothesis on the direct experiment.

tion in the airs, which was soon followed by the glass vessel becoming hot. The heat gradually pervaded the glass, and was dissipated in the circumambient air, and as the glass grew cool, a mist or visible vapour appeared in it, which was condensed on the glass in the form of moisture or dew *. When the glass was cooled to the temperature of the atmosphere, if the vessel was opened with its mouth immersed in water or mercury, so much of these liquids entered, as was sufficient to fill the glass within about $\frac{1}{100}$ th part of its whole contents; and this small residuum may safely be concluded to have been occasioned by some impurity in one or both kinds of air. The moisture adhering to the glass, after these deflagrations, being wiped off, or sucked up, by a small piece of sponge paper, first carefully weighed, was found to be exactly, or very nearly, equal in weight to the airs employed."

"In some experiments, but not in all, a small quantity of a sooty-like matter was found adhering to the inside of the glass," the origin of which is not yet investigated; but Dr. PRIESTLEY thinks, that it arises from some minute grains of the mercury that was used in order to fill the glass with the air, which being super-phlogisticated by the inflammable air, assumed that appearance; but, from whatever cause it proceeded, "the whole quantity of sooty-like matter was too small to be an object of consideration, particularly as it did not occur in all the experiments."

I am obliged to your friendship for the account of the experiments which have been lately made at Paris on this subject.

* I believe that Mr. CAVENDISH was the first who discovered that the combustion of dephlogisticated and inflammable air produced moisture on the sides of the glass in which they were fired.

with

with large quantities of these two kinds of air, by which the essential point seems to be clearly proved, that the deflagration or union of dephlogistified and inflammable air, by means of ignition, produces a quantity of water equal in weight to the airs; and that the water, thus produced, appeared, by every test, to be pure water. As I am not furnished with any particulars of the manner of making the experiment, I can make no observations on it, only that, from the character you give me of the gentlemen who made it, there is no reason to doubt of its being made with all necessary precautions and accuracy, which was farther secured by the large quantities of the two airs consumed.

3. "Let us now consider what obviously happens in the case of the deflagration of the inflammable and dephlogistified air. These two kinds of air unite with violence, they become red-hot, and upon cooling totally disappear. When the vessel is cooled, a quantity of water is found in it equal to the weight of the air employed. This water is then the only remaining product of the process, and *water, light, and heat*, are all the products," unless there be some other matter set free which escapes our senses.

"Are we not then authorised to conclude, that water is composed of dephlogistified air and phlogiston, deprived of part of their latent or elementary heat; that dephlogistified or pure air is composed of water deprived of its phlogiston, and united to elementary heat and light; and that the latter are contained in it in a latent state, so as not to be sensible to the thermometer or to the eye; and if light be only a modification of heat, or a circumstance attending it, or a component part of the inflammable air, then pure or dephlogistified air is composed of water deprived of its phlogiston and united to elementary heat?"

4. "It

4. "It appears, that dephlogisticated water," or, which may be a better name for the basis of water and air, the element you call *humor*, "has a more powerful attraction for phlogiston than it has for latent heat, but that it cannot unite with it, at least not to the point of saturation, or to the total expulsion of the heat, unless it be first made red-hot," or nearly so. "The electric spark heats a portion of it red-hot; the attraction between the humor and the phlogiston takes place, and the heat which is let loose from this first portion heats a second, which operates in a like manner on the adjoining particles, and so continually until the whole is heated red-hot and decomposed." Why this attraction does not take place to the same degree in the common temperature of the atmosphere, is a question I am not yet able to solve; but it appears, that, in some circumstances, "dephlogisticated air can unite, in certain degrees, with phlogiston without being changed into water." Thus Dr. PRIESTLEY has found, that by taking clean filings of iron, which, alone, produce only inflammable air of the purest kind, and *mercurius calcinatus per se*, which gives only the purest dephlogisticated air, and exposing them to heat, in the same vessel, he obtained neither dephlogisticated nor inflammable air, "but in their place fixed air." Yet it is well known, that a mixture of dephlogisticated and inflammable air will remain for years in close vessels in the common heat of the atmosphere, without suffering any change, the mixture being as capable of deflagration at the end of that time as it was when first shut up. These facts the Doctor accounts for, by supposing that the two kinds of air, when formed at the same time in the same vessel, can unite in their *nascent* state; but that, when fully formed, they are incapable of acting upon one another, unless they are first

first set in motion by external heat. "Phlogisticated air seems also to be another composition of phlogiston and dephlogisticated air;" but in what proportions they are united, or by what means, is still unknown. It appears to me to be very probable, that fixed air contains a greater quantity of phlogiston than phlogisticated air does, because it has a greater specific gravity, and because it has more affinity with water.

5. "For many years I have entertained an opinion, that air was a modification of water, which was originally founded on the facts that in most cases, wherein air was actually made," which should be distinguished from those wherein it is only extricated from substances containing it in their pores, or otherwise united to them in the state of air, "the substances were such as were known to contain water as one of their constituent parts, yet no water was obtained in the processes," except what was known to be only loosely connected with them, such as the water of the crystallization of salts. "This opinion arose from a discovery," that the latent heat contained in steam diminished in proportion as the sensible heat of the water from which it was produced increased; or, in other words, "that the latent heat of steam was less when it was produced under a greater pressure, or in a more dense state, and greater when it was produced under a less pressure, or in a less dense state; which led me to conclude, that when a very great degree of heat was necessary for the production of the steam, the latent heat would be wholly changed into sensible heat; and that, in such cases, the steam itself might suffer some remarkable change. I now abandon this opinion in so far as relates to the change of water into air, as I think that may be accounted for on better principles."

6. "In every case, wherein dephlogisticated air has been produced, substances have been employed, some of whose constituent parts have a strong attraction for phlogiston, and, as it would appear, a stronger attraction for that substance than *humor* has; they should, therefore, dephlogisticate the water" or fixed air, and the *humor* thus set free should unite to the matter of fire and light and become pure air. Dephlogisticated air is produced in great abundance from melted nitre. The acid of nitre has a greater attraction for phlogiston than any other substance is known to have; and it is also certain, that nitre, besides its water of crystallization, contains a quantity of water as one of its elementary parts, which water adheres to the other parts of the nitre with a force sufficient to enable it to sustain a red heat. When the nitre is melted, or made red-hot, the acid acts upon the water and dephlogisticates it; and the fire supplies the *humor* with the due quantity of heat to constitute it air, under which form it immediately issues. It is not easy to tell what becomes of the acid of nitre and phlogiston, which are supposed to be united," as they seem to be lost in the process. Dr. PRIESTLEY has lately made some experiments, with a view to ascertain this point. He distilled dephlogisticated air from pure nitre, in an earthen retort glazed within and without. He employed 2 oz. = 960 grains of nitre; the retort was placed in an air furnace, and, by means of an intense heat, he obtained from the nitre in one experiment 787, and in another experiment 800 ounce measures of dephlogisticated air; and he found that, upon weighing the retort and nitre before and after the process, they had suffered a loss of weight equal to the weight of the air, and to the water of crystallization of the nitre, but nothing more. He remarked, that the air had a pungent smell,

smell, which he could not divest it of by washing; and that the water in which the air was received had become slightly acid. I examined a portion of this water, which he was so kind as to send me, and found by it that the whole of the receiving water had contained the acid belonging to 2 drams = 120 grains of nitre. I also examined the residuum and the retort in which the distillation had been performed, and found the residuum highly alkaline, yet containing a minute quantity of phlogisticated nitrous acid. It had acted considerably upon the retort, and had dissolved a part of it, which was deposited in the form of a brownish powder, when the saline part was dissolved in water. This earthy powder I have not yet thoroughly examined, but have no doubt that it principally consists of the earth of the retort. This experiment, and all others tried in earthen vessels, leave us still at a loss to determine what becomes of the acid and phlogiston. They seem either to remain mixed with the air, in the form of an incoercible gas; or to unite with the alkali, or with the earth of the retort, in some manner so as not to be easily separated from them; or else they are imbibed by the retorts themselves, which are sufficiently porous to admit of such a supposition.

All that appears to be conclusive from this experiment is, that above one half of the weight of the nitre was obtained in the form of dephlogisticated air; and that the residuum still contained some nitrous acid united to phlogiston.

7. Finding that the action of the nitre on the retort tended to prevent any accurate examination of the products, I had recourse to combinations of the nitrous acid with earths from which the dephlogisticated air is obtained with less heat than from nitre itself. As these processes have been particularly described by Dr. PRIESTLEY, by Mr. SCHEELE, and others, I

shall not enter into any detail of them; but shall mention the general phænomena which I observed, and which relate to the present subject.

The earths I used were magnesia alba, calcareous earth, and minium or the red calx of lead. I dissolved them in the respective experiments in nitrous acid dephlogisticated by boiling, and diluted with proper proportions of water. I made use of glass retorts, coated with clay; and I received the air in glass vessels, whose mouths were immersed in a glazed earthen basin, containing the smallest quantity of water that could be used for the purpose. As soon as the retort was heated a little above the heat of boiling water, the solutions began to distil watery vapours containing nitrous acid. Soon after these vapours ceased, yellow fumes, and in some of the cases dark red fumes, began to appear in the neck of the retort; and at the same time there was a production of dephlogisticated air, which was greater in quantity from some of these mixtures than from others, but continued in all of them until the substances were reduced to dryness. I found, in the receiving water &c. very nearly the whole of the nitrous acid used for their solution, but highly phlogisticated, so as to emit nitrous air by the application of heat; and there is reason to believe, that with more precaution the whole might have been obtained.

8. As the quantity of dephlogisticated air produced by these processes did not form a sufficient part of the whole weight, to enable me to judge whether any of the real acid entered into the composition of the air obtained, I ceased to pursue them further, having learned from them the fact, *that however much the acid and the earths were dephlogisticated before the solution, the acid always became highly phlogisticated in the process.*

In

In order to examine whether this phlogiston was furnished by the earths, some dephlogisticated nitrous acid was distilled from minium till no more acid or air came over. More of the same acid was added to the minium as soon as it was cold, and the distillation repeated, which produced the same appearance of red fumes and dephlogisticated air. This operation was repeated a third time on the same minium, without any sensible variation in the phenomena. The process should have been still farther repeated, but the retort broke about the end of the third distillation. The quantity of minium used was 120 grains, and the quantity of nitrous acid added each time was 240 grains, of such strength that it could dissolve half its weight of mercury, by means of heat.

It appears from this experiment, that unless minium be supposed to consist principally of phlogiston, the source of the phlogiston, thus obtained, was either the nitrous acid itself, or the water with which it was diluted; or else that it came through the retort with the light, for the retort was in this case red-hot before any air was produced; yet this latter conclusion does not appear very satisfactory, when it is considered, that in the process wherein the earth made use of was magnesia, the retort was not red-hot, or very obscurely so, in any part of the process; and by no means luminous, when the yellow and red fumes first made their appearance.

9. As the principal point in view was to determine whether any part of the acid entered into the composition of the air, I resolved to employ some substance which would part with the acid in a moderate heat, and also give larger quantities of air than had been obtained in the former processes. Mercury was thought a proper substance for this purpose. 240 grains of mercury were put into a glass retort with 480 grains

of diluted dephlogisticated nitrous acid, which was the quantity necessary to dissolve the whole of the mercury, a gentle heat was applied, and as soon as the common air contained in the retort was dissipated, a vessel was placed to receive the nitrous air proceeding from the solution, which was 16 ounce measures. When it had ceased to give nitrous air, the neck of the retort became hot from the watery steams of the acid. The air receiver was taken away, and a common receiver was luted on, with a little water in it, to condense the vapours, and a quantity of dilute, but highly phlogisticated, acid was caught in the receiver. When the watery vapours had nearly come over, and yellow fumes appeared in the neck of the retort, the common receiver was removed, and the air receiver replaced; about four ounces of very strong nitrous air passed up immediately, the fumes in the retort became red, and dephlogisticated air passed up, which, uniting with the nitrous air in the receiver, produced red fumes in the receiver; and the two kinds of air acting upon one another, their bulk was reduced to half of an ounce measure. At this period the fumes in the retort were of a dark red colour, and dephlogisticated air was produced very fast. After a short time, some orange-coloured sublimate appeared in the upper part of the retort, and extended a little way along its neck, the red colour of the fumes gradually disappeared, and the neck of the retort became quite clear. At the same time that this happened, small globules of mercury appeared in the neck of the retort, and accumulated there until they ran down in drops. The production of the air was now very rapid, and accompanied with much of the white cloud or powdery matter, which passed up with the air into the receiver, and mixed with the water, but did not dissolve in it. After giving about 36 ounce measures of dephlogisticated air, it

It suddenly ceased to give any more; and the retort being cooled, the bulb was found to be quite empty, excepting a small quantity of black powder, which, on being rubbed on the hand, proved to be mostly running mercury. The orange-coloured sublimate was washed out of the neck of the retort, and what running mercury was in it was separated, and added to that which had run down into the basin among the water. The whole fluid mercury, when dried, weighed 218 grains; therefore 22 grains remained in the form of sublimate, which, I believe, would also have been reduced if I could have applied heat in a proper manner to the neck of the retort, as some of it, to which heat could be applied, disappeared.

10. The 16 ounce measures of nitrous air, which had been produced in the solution of the mercury, and had remained confined by water in the receiver, was converted into nitrous acid by the gradual admission of common air, and was taken up by the water; this water was added to that in the basin, which had served to receive the dephlogisticated air. The whole quantity was about two quarts, was very acid to the taste, and sparkling with nitrous air. It was immediately put into bottles, and well corked, until it had lost the heat gained in the operation. In order to determine the quantity of acid in the receiving water and in the sublimate, I dissolved, first, alkali of tartar in water, and filtered the solution. 352 grains of this alkaline solution saturated 120 grains of the nitrous acid I had employed to dissolve the mercury, and 1395 grains of the same alkaline solution saturated the orange-coloured precipitate, and all the acid liquor obtained from the process: therefore we have the proportion as $352 : 120 :: 1395 : 475$, from which it appears, that all the acid employed was recovered again in the form of acid, excepting only five grains; a smaller.

a smaller quantity than what might reasonably be supposed to be lost in the process by the extreme volatility of the nitrous air. In order to ascertain the exact point of saturation, slips of paper, stained by the juice of the petals of the scarlet rose, were employed, which were the nicest test I could procure, as litmus will not shew the point of saturation of any liquor containing much phlogisticated nitrous acid, or even fixed air, but will turn red, and shew it to be acid, when the test of those leaves, violets, and some other of the like kind, will turn green in the same liquor, and shew it to be alkaline. But the exact point of saturation of so dilute a liquor is so very difficult to ascertain, than an error might easily be committed, notwithstanding the attention bestowed upon it. Supposing this experiment to be unexceptionable, the conclusions which may be drawn from it are very favourable to the hypothesis I endeavour to support. *Thirty-six ounce measures of dephlogisticated air were obtained, and only five grains of a weak nitrous acid were lost in the process. Two hundred and eighteen grains of mercury out of two hundred and forty were revived, and all the dephlogisticated nitrous acid employed is found to be highly phlogisticated in the process. It appears, that the nitrous acid does not enter into the composition of dephlogisticated air; it seems only to serve to absorb phlogiston from the watery part of the mercurial nitre.*

11. As this last process proved very tedious and complicated on account of the necessity of ascertaining the quantity of acid in the receiving water, by means of an alkali which afforded a double source of error in the point of saturation, I resolved to try the distillation of dephlogisticated air from cubic nitre in a glass vessel, and to draw from it only such a quantity of air as it would yield without acting much upon the retort, which latter

latter circumstance is essentially necessary to be attended to. An ounce of the crystals of mineral alkali were dissolved in nitrous acid, and the mixture brought to an exact saturation by the test of litmus; 30 ounce measures of air were distilled from it, which, during the latter part of the process, was accompanied with slightly yellow fumes; the receiving water was found to be acid, and the residuum alkaline. The residuum being dissolved in the receiving water, the solution was neutral, or very nearly so, by every test; for in this case litmus might be used, as the acid was very slightly phlogificated. On adding a few drops of a very dilute nitrous acid, the tests shewed the liquor to be acid.

12. Encouraged by the success of this experiment, I took an ounce = 480 grains of pure common nitre, and put it into a flint-glass retort, coated, which was placed in a furnace. It began to give air about the time it became red-hot, and during the latter part of the process this air was accompanied with yellowish fumes. I stopped the process when it had produced 50 ounce measures of air. The receiving water, and particularly the air, had a strong but peculiar smell of nitrous acid. The air was well washed with the receiving water, but was not freed from the smell. The receiving water, which was 50 ounces, was slightly acid, and the residuum alkaline. I dissolved the latter in the former, and found the mixture alkaline. 10 grains of weak nitrous acid were added to it, which saturated it, and 105 grains of this spirit of nitre was found to contain the acid of 60 grains of nitre; therefore the 10 grains contained the acid of 5.7 grains of nitre, which, by Mr. KIRWAN's experiments is equal to two grains of real nitrous acid. *We have, therefore, 34 grains weight of dephlogificated air produced, and only two grains of real acid missing; and it is not certain*

certain that this quantity was destroyed, because some portion of the glass of the retort was dissolved by the nitre, and some part of the materials employed in making the glass being alkali, we may conclude, that the alkali of the nitre would be augmented by the alkali of that part of the glass it had dissolved. As the glass cracked into small pieces on cooling, and some part of the coating adhered firmly to it, the quantity of the glass that was dissolved could not be ascertained. *From this experiment it appears, that if any of the acid of the nitre enters into the composition of the dephlogisticated air, it is a very small part; and it rather seems, that the acid, or part of it, unites itself so firmly to the phlogiston as to lose its attraction for water.*

13. "The vitriolic salts also yield dephlogisticated air by heat; and in these cases the dephlogisticated air is always attended with a large quantity of vitriolic acid air or sulphureous vapour," even when the salts used are not known to contain any phlogistic matter. Mr. SCHEELÉ mentions his having obtained dephlogisticated air from manganese dissolved in acid of phosphorus, and also from the arsenical acid: from whence it appears, that these acids, or perhaps any acid which can bear a red heat, can concur to the production of dephlogisticated air. *It is necessary to remark, that no experiments have been yet published shewing that dephlogisticated air can be produced from salts formed by the muriatic acid. The acids which produce salts suitable for this purpose, have all a strong affinity with phlogiston; and the marine acid has either a very small affinity with it, or else is already saturated with it, at least so far saturated as not to be able to attract it from the humor.*

14. "The dephlogisticated air obtained from the pure calces of metals may be attributed to the calces themselves, attracting the phlogiston from water which they have imbibed from
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“ the atmosphere, or from dephlogisticating the fixed air which
“ they are known to contain.”

It is very probable, that the dephlogisticated air extruded from growing vegetables may be owing to their dephlogisticating the water they grow in; but it appears more probable, that the plants have a power of dephlogisticating the fixed, or phlogisticated, air of the atmosphere.

“ When dephlogisticated and nitrous air are mixed, the dephlogisticated air seizes part of the phlogiston of the nitrous air.” The water contained in the nitrous air, and the other part of the phlogiston, unite with the nitrous acid, which then assumes a liquid form, or at least that of a dense vapour; “ and
“ that part of the latent heat of the two airs not essential to the
“ new combination is set at liberty *.”

In the combustion of sulphur the same thing happens, but in a greater degree; for the vitriolic acid, having a much weaker attraction for phlogiston than air has, abandons it almost entirely to the latter, which is thereby converted into water, and in that form attracts the vitriolic acid, and reduces it to a liquid state. The same reasoning may be applied to the combustion of phosphorus, which is attended with similar effects.

* I cannot take upon me to determine, from any facts which have come to my knowledge, whether any part of the dephlogisticated air employed in this experiment is turned into fixed air; but I am rather inclined to think that some part is, because the quantity of heat, which is separated by the union of the two airs, does not seem to be so great as that which is separated when the dephlogisticated air is wholly changed into water: yet some water appears to be formed, because when the mixture is made over mercury, the solution of the mercury in the nitrous acid assumes a crystallized form, which, however, may be due to the watery part of the nitrous air.

15. I shall not make, at present, any further deductions from what I myself consider still in the light of a conjectural hypothesis, which I have perhaps dwelt upon too long already. I shall only beg your attention to some general reasoning on the subject; which, however, may possibly serve more to shew the uncertainty of other systems on the constituent parts of air, than the certainty of this. Some of those systems suppose dephlogisticated air to be composed of an acid and something else, some say phlogiston. If an acid enters into the composition of it, why does not that acid appear again when the air is decomposed, by means of inflammable air and heat? And why is the water which is the product of this process pure water? And if an acid forms one of its constituent parts, why has nobody been able to detect any difference in the dephlogisticated air, made by the help of different acids, when compared with one another, or with the air extruded by vegetables? These airs, of such different origins, appear to be exactly the same. And if phlogiston be a constituent part of air, why does it attract phlogiston with such avidity? Some have, on the other hand, contended that air is composed of earth, united to acids or phlogiston, or to both, or to some other matter. Here we must ask, what earth it is which is one of the component parts of air? All earths which will unite with the nitrous or vitriolic acids, and with some others, such as the phosphoric and the arsenical acids, will serve as bases for the formation of air, and the air produced from all of them appears by every test to be the same, when freed from accidental impurities. To this argument it is answered, that it is not any particular species of earth which is the basis of air, but elementary or simple earth, which is contained in all of them. If this were the matter

matter of fact, would not that earth be found after the decomposition of the air?

Mr. SCHEELE has formed an hypothesis on this subject, in which he supposes heat to be composed of dephlogisticated air united to phlogiston, and that this combination is sufficiently subtile to pass through glass vessels. He affirms, that the nitrous and other acids, when in an ignited state, attract the phlogiston from the heat, and set the dephlogisticated air at liberty; but he does not seem to have been more successful than myself in explaining what becomes of the acid of nitre and phlogiston in the case of the decomposition of nitre by heat. And since we know, from the late experiments, that water is a composition of air, or more properly, *humor* and phlogiston, his whole theory must fall to the ground, unless that fact be otherwise accounted for, which it does not seem easy to do.

16. To return to the experiment of the deflagration of dephlogisticated and inflammable air, "it appears from the
"two airs becoming red-hot on their union, that the quantity
"of heat contained in one or both of them, is much greater
"than that contained in steam; because, for the first moments
"after the explosion, the water deposited by the air remains in
"the form of steam, and consequently retains the latent heat
"due to that modification of water. This matter may be easily
"examined by firing the mixture of dephlogisticated and inflammable air in a vessel immersed in another vessel containing a
"given quantity of water of a known heat, and after the vessel
"in which the deflagration is performed is come to the same
"temperature with the water in which it is immersed, by examining how much heat that water has gained, which being
"divided by the quantity of water produced by the decomposition of the airs, will give the whole quantity of elemen-

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"tary or latent heat which that water had contained, both as
 "air and as steam; and if from that quantity we deduct the
 "latent heat of the steam, the remainder will be the latent or
 "elementary heat contained more in air than in steam." This
 experiment may be made more compleatly by means of the ex-
 cellent apparatus which Mess. LAVOISIER and DE LA PLACE
 have contrived for similar purposes.

Until direct experiments are made, we may conclude, from
 those which have been made by the gentlemen just named, on
 the decompositions of air by burning phosphorus and char-
 coal, that the heat extricated during the combustion of inflam-
 mable and dephlogisticated air is much greater than it appears
 to be; for they found that one Paris ounce (= 576 Parilian
 grains) of dephlogisticated air, when decomposed by burning
 phosphorus, melted 68,634 ounces of ice; and as, according to
 another of their experiments, ice, upon being melted, absorbs
 135° of heat, by FAHRENHEIT's scale, each ounce of air gave
 out $68,634 \times 135^{\circ} = 9265^{\circ},590$; that is to say, a quantity of
 heat which would have heated an ounce of water, or any other
 matter which has the same capacity for receiving heat as water
 has, from 32° to $9265\frac{1}{2}^{\circ}$: a surprising quantity! (It is to be
 understood, that all the latent heats mentioned herein are com-
 pared with the capacity of water). And when an ounce of
 dephlogisticated air was changed into fixed air, by burning
 charcoal, or by the breathing of animals, it melted 29,547 oz.
 of ice; consequently we have $29,547 \times 135^{\circ} = 3988^{\circ},845$. the
 quantity of heat which an ounce of dephlogisticated air loses
 when it is changed into fixed air. By the heat extricated dur-
 ing the detonation of one ounce of nitre with one ounce of
 sulphur, 32 ounces of ice were melted; and, by the experiment
 I have mentioned of Dr. PRIESTLEY's (6), it appears that

nitre can produce one-half of its weight of dephlogificated air: When the nitre and sulphur are kindled, the dephlogificated air of the nitre unites with the phlogiston of the sulphur, and sets its acid free, which immediately unites to the alkali of the nitre, and produces vitriolated tartar. The dephlogificated air, united to the phlogiston, is turned into water, part of which is absorbed by the vitriolated tartar, and part is dissipated in the form of vapours, or unites to the nitrous air, or other air, produced in the process.

As half an ounce of dephlogificated air is, in this process, united by inflammation to a quantity of phlogiston sufficient to saturate it, and no fixed air is produced, it should melt a quantity of ice equal to the half of what was melted by the combination of an ounce of air with phlogiston in burning phosphorus; that is, it should melt 34.317 ounces of ice: and we find, by Messrs Lavoisier and Berthollet's experiment, that it actually melted 32 ounces of ice: the small difference may be accounted for by supposing, that the heat produced by the combustion might not be quite so great as that Dr Priestley employed in his experiment; or that the nitre might be less pure, and consequently not so much air formed. The two facts, however, agree near enough to permit us to conclude, that dephlogificated air, in uniting to the phlogiston of sulphur, produces as much heat as it does in uniting with the phlogiston of phosphorus.

17. According to Dr Priestley's experiments, dephlogificated air unites completely with about twice its bulk of the inflammable air from metals. The inflammable air being supposed to be wholly phlogiston, and being $\frac{1}{12}$ of the weight of an equal bulk of dephlogificated air, and being double in quantity, will be $\frac{1}{4.8}$ of the weight of the dephlogificated air.

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it unites with. Therefore one ounce (576 grains) of dephlogisticated air, will require 120 grains of inflammable air, or phlogiston, to convert it into water. And supposing the heat extricated by the union of dephlogisticated and inflammable air to be equal to that extricated by the burning of phosphorus, we shall find, that the union of 120 grains of inflammable air with 576 grains of dephlogisticated air, extricates 9265° of heat.

18. In the experiment on the deflagration of nitre with charcoal, by Mess. LAVOISIER and DE LA PLACE, an ounce of nitre and one third of an ounce of charcoal melted twelve ounces of ice. Supposing the ounce of nitre to have produced half an ounce of dephlogisticated air, it ought to have consumed 0,1507 ounces of charcoal, and should have melted 14,773 ounces of ice; and I suppose it fell short of its effect by the heat not being sufficiently intense to decompose the nitre perfectly.

19. By the above gentlemen's experiment an ounce of charcoal required for its combustion 3,3167 ounces of dephlogisticated air, and produced 3,6715 ounces of fixed air; therefore there was united to each ounce of air, when changed into fixed air, 61,5 grains of phlogiston, and 3988° of heat were extracted. *It appears by these facts, that the union of phlogiston, in different proportions, with dephlogisticated air, does not extricate proportional quantities of heat.* For the addition of 61,5 grains produces 3988°. and the union of 120 grains produces 9265°. This difference may arise from a mistake in supposing the specific gravity of the inflammable air Dr. PRIESTLEY employed to have been only $\frac{1}{9,6}$ of that of dephlogisticated air; for if it be supposed that its specific gravity was a little more than $\frac{1}{4}$ of that of the dephlogisticated air, then equal additions of phlogiston would have

have produced equal quantities of heat* : this matter should therefore be put to the test of experiment, by deflagrating dephlogisticated air with inflammable air of a known specific gravity, or by finding how much dephlogisticated air is necessary for the combustion of an ounce of sulphur, the quantity of phlogiston in which has been accurately determined by Mr. KIRWAN; or by finding the quantity of phlogiston in phosphorus, the quantity of dephlogisticated air necessary for its decomposition being known from Mess. LAVOISIER and DE LA PLACE's experiments.

1. On considering these latter gentlemen's experiments on the combustion of charcoal, a difficulty arises, to know what became of the remainder of the ounce of charcoal; for the dephlogisticated air, in becoming fixed air, gained only the weight of 0,3548, or about $\frac{1}{3}$ of an ounce; about $\frac{2}{3}$ of an ounce are therefore unaccounted for. The weight of the ashes of an ounce of charcoal is very inconsiderable; and, by some experiments of Dr. PRIESTLEY's, charcoal, when freed from fixed air, and other air which it imbibes from the atmosphere, is almost wholly convertible into phlogiston. The cause of this apparent loss of matter, I doubt not, these gentlemen can explain satisfactorily, and very probably in such a manner as will throw other lights on the subject.

* Or it may arise from my being mistaken, in supposing that the same quantity of heat is disengaged by the union of dephlogisticated air with phlogiston, in the form of inflammable air, as is by its union with the phlogiston of phosphorus or sulphur; and there appears to be some reason why there should not; because in these latter cases the water, being united to the acids, cannot retain so much elementary heat as it can do when left in the form of pure water, which is the case when the inflammable air is used.

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It is also worthy of enquiry, whether all the amazing quantity of heat let loose in these experiments was contained in the dephlogisticated air; or whether the greatest portion of it was not contained in the phlogiston or inflammable air. If it was all contained in the dephlogisticated air, "*the general rule is not a fact, that elastic fluids are enlarged in their dimensions in proportion to the quantity of heat they contain;*" because then, inflammable air, which is ten times the bulk of dephlogisticated air, must be supposed to contain no heat at all; "and it is known, from some experiments of my friend Dr. BLACK'S, and some of my own, that the steam of boiling water, whose latent and sensible heat are only 1100°, reckoning from 60°, or temperature, is more than twice the bulk of an equal weight of dephlogisticated air." It seems, however, reasonable to suppose, that the greater quantity of heat should be contained in the rarer fluid.

It may be alledged, that in proportion to the quantity of phlogiston that is contained in any fluid, the quantity of heat is lessened. But if we reason by analogy, the attraction of the particles of matter to one another in other cases is increased by phlogiston, and "bodies are thereby rendered specifically heavier;" and we know of no other substance besides heat which can be supposed to separate the particles of inflammable air, and to endow it with so very great an elastic power, and so small a specific gravity. On the other hand, if a great quantity of elementary heat be allowed to be contained in inflammable air, on account of its bulk, the same reasoning cannot hold good in respect to the phlogiston of phosphorus, sulphur, charcoal, &c. But all these substances contain other matters besides phlogiston and heat. The acids in the sulphur and

and phosphorus, and the alkali and earth in charcoal, may attract the phlogiston so powerfully that the heat they contain may not be able to overcome the adhesion of their particles, until, by the effect of external heat, they are once removed to such a distance from one another as to be out of the sphere of that kind of attraction *.

If it be found to be a constant fact, that equal additions of phlogiston to dephlogisticated air do not extricate equal quantities of heat, that may afford the means of finding the quantities of heat contained in phlogiston and dephlogisticated air respectively, and solve the problem.

Many other ideas on these subjects present themselves; but I am not bold enough to trouble you, or the public, with any speculations, but such as I think are supported by uncontroverted facts.

I must therefore bring this long letter to a conclusion, and leave to others the future prosecution of a subject which, however engaging, my necessary avocations prevent me from pursuing. I cannot however conclude, without acknowledging my obligations to Dr. PRIESTLEY, who has given me every information and assistance in his power, in the course of my enquiries, with that candour and liberality of sentiment which distinguish his character.

I return you my thanks for the obliging attention you have paid to this hypothesis; and remain, with much esteem, &c.

JAMES WATT.

* On the whole, this question seems to involve so many difficulties, that it cannot be cleared up without many new experiments.



XXVI. *Sequel to the Thoughts on the constituent Parts of Water and Dephlogisticated Air. In a subsequent Letter from Mr. James Watt, Engineer, to Mr. De Luc, F. R. S.*

Read May 6, 1784.

DEAR SIR,

Birmingham,
April 30, 1784.

ON re-considering the subject of my letter to you of the 26th of November last, I think it necessary to resume the subject, in order to mention some necessary cautions to those who may chuse to repeat the experiments mentioned there, and to point out some circumstances that may cause variations in the results.

In experiments where the dephlogisticated air is to be distilled from common or cubic nitre, these salts should be purified as perfectly as possible, both from other salts and from phlogistic matter of any kind; otherwise they will produce some nitrous air, or yellow fumes, which will lessen the quantity, and, perhaps, debase the quality of the dephlogisticated air. If the nitre is perfectly pure, no yellow fumes are perceptible, until the alkaline part begins to act upon the glass of the retort, and even then they are very slightly yellow.

When earthen retorts are used, and a large quantity of air is drawn from the nitre, it acts very much upon the retort, dissolves a great part of it, and becomes very alkaline, retaining only a small part of its acid, at least only a small part which

can be made appear in any of the known forms of that acid; and unless retorts can be obtained of a true apyrous and compact porcelain, I should prefer glass retorts, properly coated, for making experiments for the present purpose.

In some of my experiments the nitre was left in the retort placed in a furnace, so that it took an hour or more to cool. In these cases there was always a deficiency of the acid part, which seemed, from some appearances on the coating, either to have penetrated the hot and soft glass, by passing from particle to particle, or to have escaped by small cracks which happened in the retort during the cooling. There was the least deficiency of the acid when the distillation was performed as quickly as was practicable, and the retort was removed from the fire immediately after the operation was finished. In order to shorten the duration of the experiment, and consequently to lessen the action of the nitre on the retort, it is advisable not to distil above 50 ounce measures of dephlogisticated air from an ounce of nitre. The experiment has succeeded best when the retort was placed in a charcoal fire in a chafing-dish or open furnace; because it is easy in that case to stop the operation, and to withdraw the retort at the proper period.

When the dephlogisticated air is distilled from the nitre of mercury, the solution should be performed in the retort itself, and the nitrous air produced by the solution should be caught in a proper receiver, and decomposed by the gradual admission of common air through water; and the water, which thus becomes impregnated with the acid of the nitrous air, should be added after the process to the water through which the dephlogisticated air has passed. When the solution ceases to give any more nitrous air, the point of the tube of the retort should be raised out of the water; otherwise, by the condensation of the

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watery and acid vapours which follow, a partial exhaustion will take place, and the receiving water will rise up into the retort and break it, or at least spoil the experiment. A common receiver, such as is used in distilling spirit of nitre, should be applied, with a little water in it, to receive the acid steam; and it should be kept as cool as can conveniently be done, as these fumes are very volatile. This receiver should remain as long as the fumes are colourless; but when they appear, in the neck of the retort, of a yellow colour, it is a mark that the mercurial nitre will immediately produce dephlogisticated air; the receiver should then be withdrawn, and an apparatus placed to receive the air. The rest of the process has been sufficiently explained in my former letter.

The phlogisticated nitrous acid, saturated by an alkali, will not crystallize; and, if exposed to evaporation, even in the heat of the air, will become alkaline again, which shews the weakness of its affinity with alkalies when dissolved in water*; a farther proof of which is, that it is expelled from them by all the acids, even by vinegar (which fact has been observed by Mr. SCHEELE). I have observed, that litmus is no test of the saturation of this acid by alkalies; for the infusion of litmus added to such a mixture will turn red, when the liquor appears to be highly alkaline, by its turning the infusions of violets, rose leaves, and most other red juices, green. This does not proceed from the infusion of litmus being more sensible to the presence of acids than other tests; for I have lately discovered a test liquor (the preparation of which I mean to publish soon) which is more sensible to the presence of acids

* You have informed me, that Mr. CAVENDISH has also observed this fact; and that he has mentioned it in a paper lately read before the Royal Society; but I had observed the fact previous to my knowledge of his paper.

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than litmus is; but which turns green in the same solution of phlogisticated nitre that turns litmus red.

The unavoidable little accidents which have attended these experiments, and which tend to render their results dubious, have prevented me from relying on them as *full* proofs of the position that no acid enters into the composition of dephlogisticated air; though they give great probability to the supposition. I have, therefore, explained the whole of the hypothesis and experiments with the diffidence which ought to accompany every attempt to account for the phænomena of nature on other principles than those which are commonly received by philosophers in general. And in pursuance of the same motives it is proper to mention, that the alkali employed to saturate the phlogisticated nitrous acid, was always that of tartar which is partly mild; and I have not examined whether highly phlogisticated nitrous acid can perfectly expel fixed air from an alkali, though I know no fact which proves the contrary. It should also be examined, whether the same quantity of real nitrous acid is requisite to saturate a given quantity of alkali, when the acid is phlogisticated, as is necessary when it is dephlogisticated.

As I am informed that you have done me the honour to communicate my former letter on this subject to the Royal Society; I shall be obliged to you to do me the same favour in respect to the present letter, if you judge that it merits it.

I remain, &c.

JAMES WATTE.



XXVII. *An Attempt to compare and connect the Thermometer for strong Fire, described in Vol. LXXII. of the Philosophical Transactions, with the common Mercurial Ones. By Mr. Josiah Wedgwood, F. R. S. Potter to Her Majesty.*

Read May 13, 1784.

THIS thermometer, which I had the honour of laying before the Royal Society in May 1782, has now been found, from extensive experience, both in my manufactories and experimental enquiries, to answer the expectations I had conceived of it as a measure of all degrees of common fire above ignition: but at present it stands in a detached state, not connected with any other, as it does not begin to take place till the heat is too great to be measured or supported by mercurial ones.

What is now therefore wanting, to give us clear ideas of the value of its degrees, is, to connect it with one which long use has rendered familiar to us; so that if the scale of the common thermometer be continued indefinitely upwards as a standard, the divisions of mine may be reduced to that scale, and we may thus have the whole range of the degrees of heat brought into one uniform series, expressed in one language, and comparable in every part, from the lowest that have hitherto been produced by any artificial freezing mixtures, up to the highest that can be obtained in our furnaces, or that the materials of our furnaces and vessels can support.

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The hope of attaining this desirable and important object gave rise to the experiments which I have now the honour of communicating. How far I may have succeeded, or whether the means employed were adequate to the end proposed, is, with all deference, submitted to this illustrious Society.

This attempt is founded upon the construction and application of an intermediate measure, which takes in both the heats that are measurable by the mercurial thermometer, and a sufficient number of those that come within the province of mine to connect the two together; the manner of doing which will be apparent from the three first figures (tab. XIV.); wherein F represents FAHRENHEIT's thermometer, with a continuation of the scale; W my thermometer; and M the intermediate measure divided into any number of equal parts at pleasure.

For if the heat of boiling water, or 212 degrees of FAHRENHEIT, be communicated to M, and its measure upon M marked, as at *a*; and if the heat of boiling mercury, or 600° of FAHRENHEIT, be also communicated to M, and marked as at *b*; it is plain, that the number of degrees upon M between *a* and *b* will be equal to the interval between 212 and 600, that is, to 388° upon FAHRENHEIT.

In like manner, upon exposing M to two different heats above ignition along with my thermometer pieces, if a certain degree of my scale be found to correspond with the point *d*, and another degree of mine with the point *c*; then the interval between those two degrees upon mine must be equal to the interval *dc*; and how many of FAHRENHEIT's that interval is equivalent to will be known from the preceding comparison. Thus we can find the number of FAHRENHEIT's degrees contained in any given extent of mine, and the degree of FAHRENHEIT's with which a given point of mine coincides;
whence

whence either scale is easily reducible to the other through their whole range, whether we suppose FAHRENHEIT's continued upwards, or mine downwards.

For obtaining the intermediate thermometer different means were thought of; but the only principle which, upon attentive consideration, afforded any prospect of success, was the *expansion of metals*. This therefore was adopted, and among different methods of measuring that expansion, which either occurred to myself, or which I can find to have been practised by others, there is no one which promises either so great accuracy, or convenience in use, as a gage like that by which the thermometer pieces are measured: the utility of this gage had now been confirmed to me by experience, and the machines and long rods, which have been employed for measuring expansions on other occasions, were absolutely inadmissible here, on account of the insuperable difficulties of performing nice operations of this kind in a red heat, and of communicating a perfectly equal heat through any considerable extent.

To give a clearer idea of this species of gage, which, simple as it is, I am informed has been misunderstood by some of the readers of my former paper, a representation of one used on the present occasion is annexed in fig. 4. where ABCD is a smooth flat plate; and EF and GH two rulers or flat pieces, a quarter of an inch thick, fixed flat upon the plate, with the sides that are towards one another made perfectly true, a little further asunder at one end EG than at the other end FH; thus they include between them a long converging canal, which is divided on one side into a number of small equal parts, and which may be considered as performing the offices both of the tube and scale of the common thermometer. It is obvious, that if a body, so adjusted as to fit exactly at the wider end of
this

this canal, be afterwards diminished in its bulk by fire, as the thermometer pieces are, it will then pass further in the canal, and more and more so according as the diminution is greater; and conversely, that if a body, so adjusted as to pass on to the narrow end, be afterwards expanded by fire, as is the case with metals, and applied in that expanded state to the scale, it will not pass so far; and that the divisions on the side will be the measures of the expansions of the one, as of the contractions of the other, reckoning in both cases from that point to which the body was adjusted at first.

I is the body whose alteration of bulk is thus to be measured, which, in the present instance, is a piece of fine silver: this is to be gently pushed or slid along, towards the end FH, till it is stopped by the converging sides of the canal.

K is a little vessel formed in the gage for this particular series of experiments, the use of which will appear hereafter.

The *contraction*, which the thermometer pieces receive from fire, is a permanent effect, not variable by an abatement of the heat, and which accordingly is measured commodiously and at leisure, when the pieces are grown cold. But the *expansion* of bodies is only temporary, continuing no longer than the heat does that produced it; and therefore its quantity, at any particular degree of heat, must be measured in the moment while that heat subsists. And further, if the heated piece was applied to the cold gage, the piece would be deprived of a part of its heat on the first contact; and as the gage receives some degree of expansion from heat as well as the piece, it is plain that in this case the piece would be diminished in its bulk, and the gage enlarged, before the measurement could be taken. It is therefore necessary that both of them be heated to an exact equality; and in that state we can measure, not indeed the *true expansion* of

either, but the *excess* of the expansion of one above that of the other, which is sufficient for the present purpose, as we want only an uniform and graduated effect of fire, and it is totally immaterial whether that effect be the absolute expansion of one or the other body, or the difference of the two, provided only that its *quantity* be sufficient to admit of nice measurement.

Some difficulties occurred with respect to the choice of a proper *matter* for the gage; the essential requisites of which are, to have but little expansibility, and to bear the necessary fires without injury. All the metals, except gold and silver, would calcine in the fire: those two are indeed free from that objection, and accordingly it is of the most expansible of them that the piece is made; but if the gage also was made of the same, the measure itself would expand just as much as the body to be measured, and no expansion at all would be sensible; and though the gage was made of one of those metals, and the piece of the other, the difference between their expansions would be too small to give any satisfactory results, as more than two-thirds of the real expansion of either would be lost or taken off by the other.

For these reasons I had recourse to earthy compositions, which expand by heat much less than metallic bodies, and bear the necessary degrees of fire without the least injury. I made choice of tobacco-pipe clay, mixed with charcoal in fine powder, in the proportion of three parts of the charcoal to five of the clay by weight. By a free access of air, in the burning by which the gage is prepared for use, the charcoal is consumed, and leaves the clay extremely light and porous; from which circumstance it bears sudden alternations of cold and heat, often requisite in these operations, much better than the clay alone. Another and more important motive for the use of charcoal was,

was, that in consequence of the remarkable porosity which it produces in the clay, it would probably diminish the expansibility, by occasioning the mass to contain, under an equal surface, a much less quantity of solid or expansible matter. It may be objected to this idea, that the expansions of metals, in Mr. ELLICOTT's * and Mr. SMEATON's † experiments, do not appear to have any connection at all with their densities: but the cases are by no means parallel; for there the comparison lies between different species of matter; but here, between one and the same matter in different states of compactness. If a metal could be treated as clay is in this instance, that is, if a large bulk of any foreign matter could be blended with it, and this matter afterwards burnt out, so as to leave the metallic particles at the same distances to which they had been separated by the mixture of it, we may presume that the metal thus enlarged would not expand so much as an equal volume of the solid metal. Such at least were the ideas which determined my choice to a composition of clay and charcoal powder; and being afterwards desirous of satisfying myself whether they had any foundation in fact, I have, since the experiments were made, prepared some pieces of clay with and without charcoal, and having burnt them in the same fire, I ground them at the sides, to make them both fit exactly to the same division near the narrow end of the gage; then, examining their expansions by equal heats, I found the piece with charcoal to expand only one-third part so much as that without; and thus was fully satisfied with the composition of the gage.

To ascertain a fixed point on the scale for the divisions to be counted from, the silver piece and gage were laid together for

* Phil. Transact. vol. XLVII. p. 485.

† Ibid. vol. XLVIII. p. 612.

some time in spring water, of the temperature of 50° of FAHRENHEIT: the point which the piece went to in this cold state is that marked near the narrow end of the gage. The adjustment is re-examined at the beginning and end of every succeeding experiment, lest the repeated attrition, in sliding the piece backwards and forwards, should wear off so much from the surface of this soft metal as to occasion an error in the minute quantities here measured.

The apparatus is then exposed successively to different degrees of heat, with the piece lying always in a part of the canal at least as wide as it is expected to fill when expanded; otherwise the sides of the gage would be burst asunder by its expansion, as I experienced in some of my first trials. When the whole has received any particular degree of heat desired, the piece is cautiously and equably pushed along, till it is stopped by the convergency of the sides, of which I always find notice given me by the gage itself (which is small and light) beginning to move upon the continuance of the impulse. A flat slip of iron, a little narrower than the piece, bent down to a right-angle at one end, and fixed in a long handle at the other, makes a convenient instrument for pushing the piece forward, or drawing it back again, whilst red-hot: this instrument, at every time of using, is heated to the same degree as the piece itself.

The heat of boiling water is taken without difficulty, by keeping the apparatus in boiling water itself during a sufficient space of time for the full heat to be communicated to it. The water I made use of was a very fine spring water, which on chemical trials appeared very nearly equal in purity to that of rain or snow; and I had previously satisfied myself, by trials in the cold, that the gage and piece being wet, or under water, made no difference in the measurement. The expansion of the

silver by this heat, that is, by an increase of the heat from 50° to 212° , or a period containing 162° of FAHRENHEIT, was just 8° of the gage or intermediate thermometer M; whence one of these degrees, according to this experiment, contains just $20^{\circ}\frac{1}{2}$ of FAHRENHEIT'S. The operation was many times repeated, and the result was always precisely the same.

For the boiling heat of mercury, it was necessary to proceed in a different manner; not to convey the heat from the mercury to the instrument, but to convey it equally to them both from another body. I made a small vessel for holding the mercury in the gage itself, seen at K fig. 4 and more distinctly in fig. 5 which is a transverse section of the gage through this vessel. The plate CD, which forms the bottom of the canal, serves also for the bottom of the vessel, which is situated close to the side of the canal, and as near as could be to that part of it, in which both the silver piece, and the divisions required for this particular experiment, are contained. By this arrangement it is presumed, that all the parts concerned in the operation will receive very nearly an equal heat.

The gage, with some mercury in the vessel, was laid upon a smooth and level bed of sand, on the bottom of an iron muffle kept open at one end; the fire increased very gradually till the mercury boiled, and then continued steady, so as just to keep it boiling, for a considerable time. The boiling heat of mercury was thus found to be $27^{\circ}\frac{3}{4}$ of the intermediate thermometer, which answering to an interval of 550° of FAHRENHEIT, makes one degree of this equal to just 20° of his; a result corresponding even beyond my expectations with that which boiling water had given.

These standard heats of FAHRENHEIT'S thermometer are obtained with little difficulty on a common fire; but it is far otherwise

otherwise with the higher ones in which mine begins to apply ; and all the precautions I could take, by using a close muffle, surrounding it as equally as possible with the fuel, varying its position with respect to the draught of air, &c. proved insufficient for securing the necessary equality of heat even through the small space concerned in these experiments. Nor had I any idea, before the discovery of this thermometer, of the extreme difficulty, not to say impracticability, of obtaining, in common fires, or in common furnaces, an uniform heat through the extent even of a few inches. Incredible as this may appear at first sight, whoever will follow me in the operations I have gone through, placing accurate measures of the heat in different parts of one and the same vessel, will soon be convinced of its truth, and that he can no otherwise expect to communicate with certainty an equal heat to different pieces, than by using a fire of such magnitude as to exceed perhaps some hundreds of times the bulk of the matters required to be heated.

To such large body of fire, therefore, after many fruitless attempts in small furnaces, not a little discouraging by the irregularity of their results, I at length had recourse, fitting up for this purpose an iron oven, used for the burning-on of enamel colours upon earthen ware, about four feet long, by two and a half wide, and three feet high, which is heated by the flame of wood conducted all round it. An iron muffle, four inches wide, two inches and three quarters high, and ten inches long, containing the gage and piece, was placed in the middle of this oven, and the vacancy between them filled up with earthen ware, to increase the quantity of ignited matter, and thereby communicate the heat more equably from the oven to the muffle. In such a situation of the muffle, in the center

center of an oven more than five hundred times its own capacity, it could not well fail of being heated pretty uniformly, at least through the small space which these experiments required; nor have I found any reason to suspect that it was not so.

The gage being laid flat upon the bottom of the muffle, with the silver piece in the canal as before, some of the clay thermometer pieces were set on end upon the silver piece, with that end of each downwards which is marked to go foremost in measuring it; that is, they were in contact with the silver in that part of their surface by which their measure is afterwards ascertained. I was led to this precaution by an experiment I had made upon another occasion, in which a number of thermometer pieces having been set upright upon an earthen-ware plate, over a small fire, till the plate became red-hot, all the pieces were found diminished, some of them more than two degrees, at the lower ends which rested upon the plate, whilst the upper ends were as much enlarged, not having yet passed the stage of extension which, as observed in the former paper, always precedes the thermometric diminution: thus we see how punctually every part of the piece obeys the heat that acts upon it.

The fire about the oven was slowly increased for some hours, and kept as even and steady as possible, by an experienced fireman, under my own inspection. Upon opening a small door, which had been made for introducing the apparatus, and looking in from time to time, it was observed, that the muffle, with the adjacent parts of the oven and ware, acquired a visible redness at the same time; and in the progress of the operation, the eye could not distinguish the least dissimilarity in the aspect of the different parts; whereas in small fires, the difference not only between the two ends of the muffle, but in much less distances, is such as to strike the eye at once.

When

When the muffle appeared of a low red heat, such as was judged to come fully within the province of my thermometer, it was drawn forward, towards the door of the oven; and its own door being then nimbly opened by an assistant, I immediately pushed the silver piece as far as it would go. But as the division which it went to could not be distinguished in that ignited state, the muffle was lifted out, by means of an iron rod passed through two rings made for that purpose, with care to keep it steady, and avoid any shake that might endanger the displacing of the silver piece.

When grown sufficiently cold to be examined, I noted the degree of expansion which the silver piece stood at, and the degree of heat shewn by the thermometer pieces measured in their own gage; then returned the whole into the oven as before, and repeated the operation with a stronger heat, to obtain another point of correspondence on the two scales.

The first was at $2^{\circ}\frac{1}{4}$ of my thermometer, which coincided with 66° of the intermediate one; and as each of these last has been before found to contain 20 of FAHRENHEIT's, the 66 will contain 1320; to which add 50, the degree of his scale to which the 0 of the intermediate thermometer was adjusted, and the sum, 1370, will be the degree of FAHRENHEIT's corresponding to my $2^{\circ}\frac{1}{4}$.

The second point of coincidence was at $6^{\circ}\frac{1}{4}$ of mine, and 92° of the intermediate; which 92 being, according to the above proportion, equivalent to 1840 of FAHRENHEIT, add 50 as before to this number, and my $6^{\circ}\frac{1}{4}$ is found to fall upon the 1890th degree of FAHRENHEIT.

It appears from hence, that an interval of 4 degrees upon mine is equivalent to an interval of 520° upon his; consequently 1 of mine to 130 degrees of his; and that the 0 of mine corresponds

to

to his $1577^{\circ}\frac{1}{4}$. Several other trials were made, which gave results so nearly alike, that I have little apprehension of any material error.

From these data it is easy to reduce either scale to the other through their whole range; and from such deduction it will appear, that an interval of near 480° remains between them, which the intermediate thermometer serves as a measure for; that mine includes an extent of about 32000 of FAHRENHEIT'S degrees; or about 54 times as much as that between the freezing and boiling points of mercury, by which mercurial ones are naturally limited; that if the scale of mine be produced downwards, in the same manner as we have supposed FAHRENHEIT'S to be produced upwards, for an ideal standard, the freezing point of water would fall nearly on 8° below 0 of mine, and the freezing point of mercury a little below $8^{\circ}\frac{1}{4}$; and that, therefore, of the extent of now measurable heat, there are about $\frac{1}{16}$ ths of a degree of my scale from the freezing of mercury to the freezing of water; 8° from the freezing of water to full ignition; and 160° above this to the highest degree I have hitherto attained.

As we are now enabled to compare not only the higher degrees among themselves, and the lower among themselves, upon their respective scales, but likewise the higher and lower with each other in every stage, it may be proper to take a general view of the whole range of measurable heat, as expressed both in FAHRENHEIT'S denominations and in mine; and for this purpose I have drawn up a little table of a few of the principal points that have been ascertained, to shew their mutual relations or proportions to each other: any other points that have been, or hereafter may be, observed, by these or any other known thermometers, may be inserted at pleasure.

	FAHR.	WEDG.
Extremity of the scale of my thermometer	32277°	240°
Greatest heat of my small air-furnace	21877	160
Cast iron melts	17977	130
Greatest heat of a common smith's forge	17327	125
Welding heat of iron, greatest	13427	95
least	12777	90
Fine gold melts	5237	32
Fine silver melts	4717	28
Swedish copper melts	4587	27
Brass melts	3807	21
Heat by which my enamel colours are burnt on	1857	6
Red-heat fully visible in day-light	1077	0
Red-heat fully visible in the dark	947	1
Mercury boils	600	3 ⁶⁷ / ₁₀₀
Water boils	212	6 ⁶⁷ / ₁₀₀
Vital heat	97	7 ¹⁴ / ₁₀₀
Water freezes	32	8 ⁶⁷ / ₁₀₀
Proof spirit freezes	0	8 ¹⁴ / ₁₀₀
The point at which mercury congeals,	} about 40	8 ²² / ₁₀₀
consequently the limit of mercurial		
thermometers,		

To assist our conceptions of this subject, it may be proper to view it in another light, and endeavour to present it to the eye; for *numbers*, on a high scale, are with difficulty estimated and compared by the mind. I have therefore completed the scales of which a part is represented in fig. 1. and 3. by continuing the same equal divisions, both upwards and downwards, as far as the utmost limits of heat that have hitherto been attained and measured*.

* Mr. WEDGWOOD presented this, in the form of a very long roll, to the Society.
In

In a scale of heat drawn up in this manner, the comparative extents of the different departments of this grand and universal agent are rendered conspicuous at a single glance of the eye. We see at once, for instance, how small a portion of it is concerned in animal and vegetable life, and in the ordinary operations of nature. From freezing to vital heat is barely a five-hundredth part of the scale; a quantity so inconsiderable, relatively to the whole, that in the higher stages of ignition, ten times as much might be added or taken away, without the least difference being discernible in any of the appearances from which the intensity of fire has hitherto been judged of. From hence, at the same time, we may be convinced of the utility and importance of a physical measure for these higher degrees of heat, and the utter insufficiency of the common means of discriminating and estimating their force. I have too often found differences, astonishing when considered as a part of this scale, in the heats of my own kilns and ovens, without being perceivable by the workmen at the time, or till the ware was taken out of the kiln.

SINCE the foregoing experiments were made, I have seen a very curious Memoir by Mess. LAVOISIER and DE LA PLACE, containing a method of measuring heat by the quantity of ice which the heated body is capable of liquefying. The application of this important discovery, as an intermediate standard measure between FAHRENHEIT's thermometer and mine, could not escape me, and I immediately set about preparing an apparatus, and making the experiments necessary for that pur-

pose; in hopes either of attaining by this method a greater degree of accuracy than I could expect from any other means; or of having what I had already done confirmed by a series of experiments upon a different principle.

But in the prosecution of these experiments I have, to my great mortification, hitherto failed of success; and I should have contented myself for the present with saying little more than this, if some phenomena had not occurred, which appear to me not unworthy of farther investigation.

The authors observe, that if ice, cooled to whatever degree below the freezing point, be exposed to a warmer atmosphere, it will be brought up to the freezing point through its whole mass before any part of its surface begins to liquefy; and that consequently ice, beginning to melt on the surface, will be always exactly of the same temperature, *viz.* at the freezing point; and that if a heated body be inclosed in a hollow sphere of such ice, the whole of its heat will be taken up in liquefying the ice; so that if the ice be defended from external warmth, by surrounding it with other ice in a separate vessel, the weight of the water produced from it will be exactly proportional to the heat which the heated body has lost; or, in other words, will be a true physical measure of the heat.

For applying these principles in practice, they employ a tin vessel, divided, by upright concentric partitions, into three compartments, one within another. The innermost compartment is a wire cage, for receiving the heated body. The second, surrounding this cage, is filled with pounded ice, to be melted by the heat; and the outermost is filled also with pounded ice, to defend the former from the warmth of the atmosphere. The first of these ice compartments terminates at bottom in a stem like a funnel, through which the water is conveyed off; and the other ice compartment terminates in a separate canal, for discharging

discharging the water into which *that* ice is reduced. As soon as the heated body is dropped into the cage, a cover is put on, which goes over both that and the first ice compartment; which cover is itself a kind of shallow vessel, filled with pounded ice, with holes in the bottom for permitting the water from this ice to pass into the second compartment; all the liquefaction that happens here, as well as there, being the effect of the heated body only. Over the whole is placed another cover with pounded ice, as a defence from external warmth.

As soon as this discovery came to my knowledge, on the 23d of February, a thaw having begun three days before, after a frost which had continued with very little intermission from the 24th of December, I collected a quantity of ice, and stored it up in a large cask in a cellar.

I thought it necessary to satisfy myself in the first place, by actual experiment, that ice, how cold soever it may be, comes up to the freezing point through its whole mass before it begins to liquefy on the surface. For this purpose I cooled a large fragment of ice, by a freezing mixture, to 17° of FAHRENHEIT's thermometer, and then hung it up in a room whose temperature was 50° . When it began to drop, it was broken, and some of the internal part nimbly pounded and applied to the bulb of a thermometer that was cooled by a freezing mixture below 30° . The thermometer rose to, and continued at, 32° ; being then taken out, and raised by warmth to 40° , some more of the same ice, applied as before to the bulb, sunk it again to 32° ; so that no doubt could remain on this subject.

Apprehensive that pounded ice, directed by the authors, might imbibe and retain more or less of the water by capillary attraction, according to circumstances, and thereby occasion some error in the results, I thought it necessary to satisfy myself in this respect
also.

also by experiment. I therefore pounded some ice, and laid it in a conical heap on a plate; and having at hand some water, coloured with cochineal, I poured it gently into the plate, at some distance from the heap: as soon as it came in contact with the ice, it rose hastily up to the top; and on lifting up the lump, I found that it held the water, so taken up, as a sponge does, and did not drop any part of it till the heat of my hand, as I suppose, began to liquefy the mass. On further trials I found, that in pounded ice pressed into a conical heap, the coloured water rose, in the space of three minutes, to the height of two inches and a half; and by weighing the water employed, and what remained upon the plate unabsorbed, it appeared, that four ounces of ice had thus taken up, and retained, one ounce of water.

To further ascertain this absorbing power, in different circumstances, more analogous to those of the process itself, I pressed six ounces of pounded ice pretty hard into the funnel, having first introduced a wooden core in order to leave a proper cavity in the middle: then, taking out the core, and pouring an ounce of water upon the ice, I left the whole for half an hour; at the end of which time the quantity that ran off was only 12 pennyweights and 4 grains, so that the ice had retained 7 pennyweights and 20 grains, which is nearly one-twelfth of its own weight, and two-fifths of the weight of the water.

These previous trials determined me, instead of using pounded ice, to fill a proper vessel with a solid mass of ice, by means of a freezing mixture, as the frost was now gone, and then expose it to the atmosphere till the surface began to liquefy. The apparatus I fitted up for this purpose was made of earthen ware well glazed, and is represented in fig. 6. (tab. XV.).

A, is

A, is a large funnel, filled with a solid mass of ice. B, a cavity in the middle of this ice, formed, part of the way, by scraping with a knife, and for the remaining part, by boring with a hot iron wire. C, one of my thermometer pieces, which serves for the heated body, and rests upon a coil of brass wire: it had previously been burnt with strong fire, that there might be no danger of its suffering any further diminution of its bulk by being heated again for these experiments. D, a cork stopper in the orifice of the funnel. E, the exterior vessel, having the space between its sides and the included funnel A, filled with pounded ice, as a defence to the ice in the funnel. F, a cover for this exterior vessel, filled with pounded ice for the same purpose. G, a cover for the funnel, filled also with pounded ice, with perforations in the bottom for allowing the water from this ice to pass down into the funnel.

The thermometer piece was heated in boiling water, taken up with a pair of small tongs equally heated, dropped instantly into the cavity B, and the covers put on as expeditiously as possible; the bottom of the funnel being previously corked, that the water might be detained till it should part with all its heat, and likewise to prevent the water from the other ice, which ran down on the outside of the funnel, from mingling with it.

After standing about ten minutes, the funnel was taken out, wiped dry, and uncorked over a weighed cup: the water that ran out weighed 22 grains. Thinking this quantity too small, as the piece weighed 72 grains, I repeated the experiment, and kept the piece longer in the funnel; but the water this time weighed only 12 grains. Being much dissatisfied with this result, I made a third trial, continuing the piece much longer in the cavity; but the quantity of water was now still less, not amounting

amounting to quite three drops; and, to my great surprise, I found the piece frozen to the ice, so as not to be easily got off, though all the ice employed was, at the beginning of the experiment, in a thawing state.

I had prepared the apparatus for taking the boiling heat of mercury; but being entirely discouraged by these very unequal results, I gave that up, for the present at least; and heating the piece to 6° of my thermometer, turned it nimbly out of the case in which it was heated into the cavity, throwing some fragments of ice over it. In about half an hour, I drew off the water, which amounted to 11 pennyweights; then stopping the funnel again, and replacing the covers, I left the whole about seven hours.

At the end of that time, I found a considerable quantity of water in the funnel: the melting of the ice had produced a cavity between it and the sides, great part of the way down, which, as well as that in the middle, was nearly full. The water nevertheless ran out so slowly, that I apprehended something had stopped the narrow end of the funnel, but the true cause became afterwards apparent upon examining the state of the ice. The fragments which I had thrown over the thermometer piece were frozen entirely together, and in such a form as they could not have assumed without fresh water superadded and frozen upon them, for the cavities between them were partly filled with new ice. I endeavoured to take the ice out with my fingers, but in vain; and it was with some difficulty I could force it asunder even, with a pointed knife, to get at the thermometer piece. When that was got out, great part of the coiled wire was found enveloped in new ice. The passage through the ice to the stem of the funnel, which I had made pretty wide with a thick iron wire red-hot, was so nearly closed

up,

up, that the flow draining off of the water was now sufficiently accounted for, and indeed this draining was the only apparent mark of any passage at all. On taking the ice out of the funnel, and breaking it to examine this canal, I found it almost entirely filled up with ice projecting from the solid mass in crystalline forms, similar in appearance to the crystals we often meet with in the cavities of flints and quartzose stones.

If, after all these circumstances, any doubt could have remained of the ice in question being a new production, a fact which I now observed must have removed all suspicion. I found a coating of ice, of considerable extent and perfectly transparent, about a tenth of an inch in thickness, upon the outside of the funnel, and on a part of it which was not in contact with the surrounding ice, for that was melted to the distance of an inch from it.

Some of the ice being scraped off from the inside of the funnel, and applied to the bulb of the thermometer, the mercury sunk from 50° to 32° , and continued at that point till the ice was melted; after which, the water being poured off, it rose in a little time to 47° .

Astonished at these appearances, of the water freezing after it had been melted, though surrounded with ice in a melting state, and in an atmosphere about 50° , where no part of the apparatus or materials could be supposed to be lower than the freezing point, I suspected at first that some of the salt of the freezing mixture might have got into the water, and that this, in dissolving, might perhaps absorb, from the parts contiguous to it, a greater proportion of heat than the ice of pure water does. But the water betrayed nothing saline to the taste, and I had applied the freezing mixture with my own hands with great care, to prevent any of it being mixed with the water.

To remove all doubts, however, upon this point, I purposed repeating the experiment with some pieces of the ice I had stored up in the cellar, to see if this would congeal, after thawing, in the same manner. But going to fetch the ice, and examining it in the cask in which it was kept, I was perfectly satisfied with the appearances I found there; for though much of it was melted, yet the fragments were frozen together, so that it was with difficulty I could break or get out any pieces of it with an iron spade; and, when so broken, it had the appearance of *breccia* marble or plum-pudding stone, for the fragments had been broken and rammed into the cask with an iron mallet.

A porcelain cup being laid upon some of this ice about half an hour, in a room whose temperature was 50° , it was found pretty firmly adhering, and when pulled off, the ice exhibited an exact impression of the fluted part of the cup which it had been in contact with; so that the ice must necessarily have liquefied first, and afterwards congealed again. This was repeated several times, with the same event. Fragments of the ice were likewise applied to one another, to sponges, to pieces of flannel and of linen cloth, both moist and dry: all these, in a few seconds, began to cohere, and in about a minute were frozen so as to require some force to separate them. After standing an hour, the cohesion was so firm, that on pulling away the fragments of ice from the woollen and sponge, they tore off with them that part of the surface which they were in contact with, though at the same time both the sponge and flannel were filled with water which that very ice had produced.

To make some estimate of the force of the congelation, which was stronger on the two bodies last mentioned than on
 5 linen,

linen, I applied a piece of ice to a piece of dry flannel which weighed two pennyweights and a half, and surrounded them with other ice. After lying together three quarters of an hour, taking the piece of ice in my hand and hooking the flannel to a scale, I found a weight of five ounces to be necessary for pulling it off, and yet so much of the ice had liquefied as to increase the weight of the flannel above 12 pennyweights. I then weighed the piece of ice, put them together again, and four hours after found them frozen so firmly as to require 78 ounces for their separation, although, from 42 pennyweights of the ice, 15 more had melted off: the surface of contact was at this time nearly a square inch. I continued them again together for seven hours; but they now bore only 62 ounces, the ice being diminished to 14 pennyweights, and the surface of contact reduced to about six-tenths of a square inch.

Having seen before that pounded ice absorbs water in very considerable quantity, I suspected that something of the same kind might take place even with entire masses; and experiment soon convinced me, that even apparently solid pieces of ice will imbibe water, slower or quicker according to its stage of decay. I have repeatedly heated some of my thermometer pieces, and laid them upon ice, in which they made cavities of considerable depth, but the water was always absorbed, sometimes as fast as it was produced, leaving both the piece and the cavity dry.

Thus, though I cannot sufficiently express how much I admire the discovery that gave rise to these experiments, I have nevertheless to lament my not being able to avail myself of it at present for the purpose I wished to apply it to.

That in my experiments the two seemingly opposite processes of nature, congelation and liquefaction, went on together, at

the same instant, in the same vessel, and even in the same fragment of ice, is a fact of which I have the fullest evidence that my senses can give me; and I shall take the liberty of suggesting a few hints, which may tend perhaps to elucidate their cause, and to shew that they are not so incompatible as at first sight they appear to be.

It occurred to me at first, that water highly attenuated and divided, as when reduced into vapour, may freeze with a less degree of cold than water in its aggregate or grosser form; hence hoar-frost is observed upon grass, trees, &c. at times when there is no appearance of ice upon water, and when the thermometer is above the freezing point*. BOERHAAVE, I find, in his elaborate theory of fire, assigns 33° as the freezing point of vapour, and even of water when divided only by being imbibed in a linen cloth.

* I am aware, that experiments and observations of this kind are not fully decisive; that the atmosphere may, in certain circumstances, be much warmer or colder than the earth and waters, which, in virtue of their density, are far more retentive of the temperature they have once received, and less susceptible of transient impressions; that even insensible undulations of water, from the slightest motion of the air, by bringing up warmer surfaces from below, may prove a further impediment to the freezing; and, therefore, that the degree of cold, which is sufficient to produce hoar-frost, may possibly, if continued long enough, be sufficient also to produce ice. I am not acquainted with any satisfactory experiments or observations yet made upon the subject; nor do I advance the principle as a certain, but as a probable one, which occurred to me at the moment, which is countenanced by general observation, and consistent to many known facts; for there are numerous instances of bodies, in an extreme state of division, yielding easily to chemical agents which, before such division, they entirely resist: thus some precipitates, in the very subtle state in which they are at first extricated from their dissolvents, are re-dissolved by other menstrua, which, after their concretion into sensible molecules, have no action upon them at all.

Now, as the atmosphere abounds with watery vapour, or water dissolved and chemically combined, and must be particularly loaded with it in the neighbourhood of melting ice; as the heated body introduced into the funnel must necessarily convert a portion of the ice or water there into vapour; and as ice is known to melt as soon as the heat begins to exceed 32° , or nearly one degree lower than the freezing point of vapour; I think we may from hence deduce, pretty satisfactorily, all the phenomena I have observed. For it naturally follows from these principles, that vapour may freeze where ice is melting; that the vapour may congeal even upon the surface of the melting ice itself; and that the heat which (agreeably to the ingenious theory of Dr. BLACK) it emits in freezing, may contribute to the further liquefaction of that very ice upon which the new congelation is formed.

I would further observe, that the freezing of water is attended with plentiful evaporation in a close as well as an open vessel, the vapour in the former condensing into drops on the under side of the cover, which either continue in the form of water, or assume that of ice or a kind of snow, according to circumstances*; which evaporation may perhaps be attributed to the heat that was combined with the water, at this moment rapidly making its escape, and carrying part of the aqueous fluid off with it. We are hence furnished with a fresh and continual source of vapour as well as of heat; so that the processes of liquefaction and congelation may go on uninterruptedly together, and even necessarily accompany one another, although, as the freezing must be in an under proportion to the melting, the whole of the ice must ultimately be consumed.

* See Mr. BARON's paper on this subject, in the Memoires of the Academy of Sciences at Paris for the year 1753.

In the remarkable instance of the coating of ice on the outside of the throat of the funnel, there are some other circumstances which it may be proper to take notice of. Neither the cover of the outer vessel, nor the aperture in its bottom which the stem of the funnel passed through, were air-tight, and the melting of the surrounding ice had left a vacancy of about an inch round that part of the funnel on which the crust had formed. As there was, therefore, a passage for air through the vessel, a circulation of it would probably take place: the cold and dense air in the vessel would descend into the rarer air of the room then about 50° , and be replaced by air from above. The effect of this circulation and sudden refrigeration of the air will be a condensation of part of the moisture it contains upon the bodies it is in contact with: the throat of the funnel, being one of those bodies, must receive its share; and the degree of cold in which the ice thaws being supposed sufficient for the freezing of this moist vapour, the contact, condensation, and freezing, may happen at the same instant.

The same principles apply to every instance of congelation that took place in these experiments; and a recollection of particulars which passed under my own eye convinces me, that the congelation was strongest in those circumstances where vapour was most abundant, and on those bodies which, from their natural or mechanic structure, were capacious of the greatest quantity of it; stronger, for instance, on sponge than on woollen, stronger on this than on the closer texture of linen, and far stronger on all these than on the compact surface of porcelain.

If, nevertheless, the principle I have assumed (that water highly attenuated will congeal with a less degree of cold than water in the mass) should not be admitted; another has above
been

been hinted at, which experiments have decidedly established, from which the phænomena may perhaps be equally accounted for, and which, even though the other also is received, must be supposed to concur for some part of the effect; I mean, that *evaporation produces cold*; both vapour and steam carrying off some proportion of heat from the body which produces them. If, therefore, evaporation be made to take place upon the surface of ice, the contiguous ice will thereby be rendered colder; and as it is already at the freezing point, the smallest increase of cold will be sufficient for fresh congelation. It seems to be on this principle that the formation of ice is effected in the East Indies, by exposing water to a serene air, at the coldest season of the year, in shallow porous earthen vessels: part of the water transudes through the vessel, and evaporating from the outside, the remainder in the vessel becomes cold enough to freeze; the warmth of the earth being at the same time intercepted by the vessels being placed upon bodies little disposed to conduct heat*. If ice is thus producible in a climate where natural ice is never seen, we need not wonder that congelation should take place where the same principle operates amidst actual ice.

It has been observed above, that the heat emitted by the congealing vapour probably unites with and liquefies contiguous portions of ice; but whether the whole, either of the heat so emitted, or of that originally introduced into the funnel, is thus taken up; how often it may unite with other portions of ice, and be driven out from other new congelations; whether there exists any difference in its chemical affinity or

* See a description of this process in the Philosophical Transactions, vol. LXV. p. 253.

elective attraction to water in different states and the contiguous bodies; whether part of it may not ultimately escape, without performing the office expected from it upon the ice; and to what distance from the evaporating surface the refrigerating effect of the evaporation may extend; must be left for further experiments to determine.

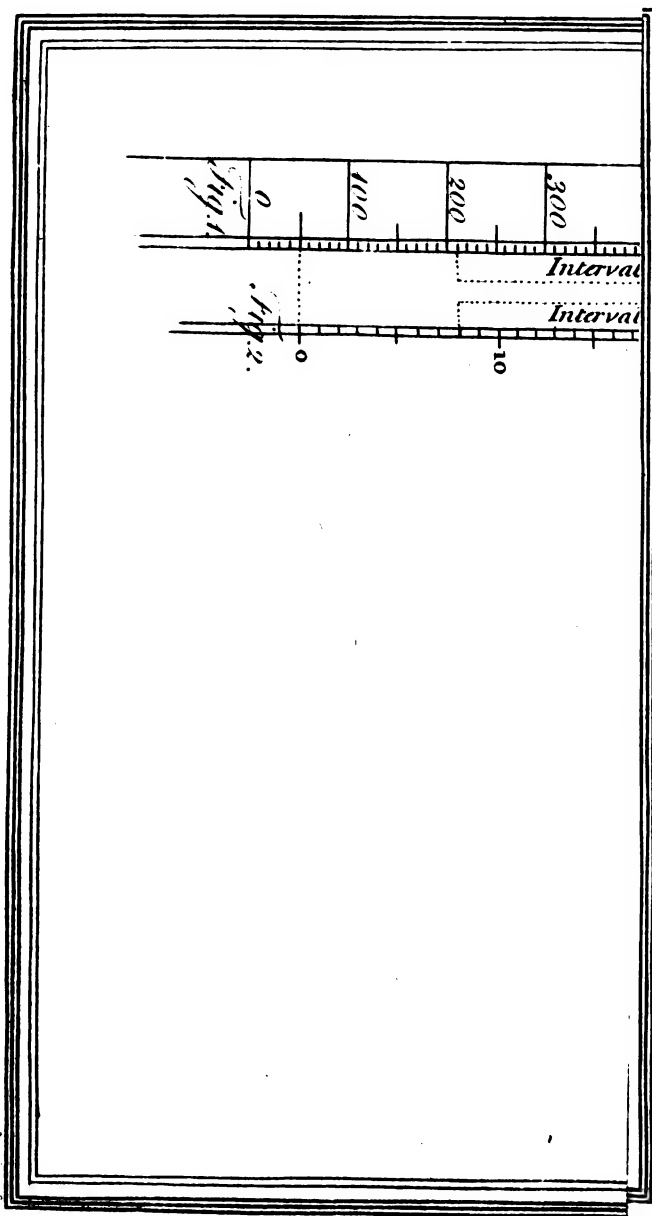


Fig. 1.

XXVIII. *On the Summation of Series, whose general Term is a determinate Function of z the Distance from the first Term of the Series.* By Edward Waring, M. D. Lucasian Professor of the Mathematics at Cambridge, and Fellow of the Societies of London and Bononia.

Read May 20, 1784.

P R O B L E M.

THE sum S being given, to find a series of which it is the sum.

1. Reduce the sum S into a converging series, proceeding according to the dimensions of any small quantities, and it is done. For example: let any algebraical function S of an unknown or small quantity x be assumed, reduce it into a converging series proceeding according to the dimensions of x , and there results a series whose sum is S . 2. Let $A, B, C, \&c.$ be algebraical functions of x ; reduce the $\int Ax, \int Bx, \int Cx, \&c.$ into a converging series, proceeding according to the dimensions of x , and the problem is done.

It is always necessary to find the values of x , between which the abovementioned serieses converge. Reduce the algebraical function S in the first example, and the algebraical functions $A, B, C, \&c.$ in the second into their lowest terms; and in such a manner, that the quantities contained in the numerator and denominator may have no denominator: make the deno-

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minator in the first example, and the denominator in the second, and every distinct irrational quantity contained in them respectively = 0; and also every distinct irrational quantity contained in the numerators = 0. Suppose α the least root affirmative or negative (but not = 0) of the abovementioned resulting equations; then a series ascending according to the dimensions of x will always converge, if the value of x is contained between α and $-\alpha$; but if x be greater than α or $-\alpha$, the abovementioned series will diverge. Let π be the greatest root of the abovementioned resulting equations; then a series descending according to the reciprocal dimensions of x will converge, if x be greater than $\pm \pi$; but, if less, not. When impossible roots $a \pm b\sqrt{-1}$ are contained in the equations, an ascending series will converge, if x be less than the least root $\pm \alpha$, and $\pm (a-b)$ and $\pm (a+b)$; or more generally, if x be less than the least root $\pm \alpha$, and x^{n+1} at an infinite distance n , be infinitely less than

$$\frac{2a^n - 2 \cdot n \cdot \frac{n-1}{2} a^{n-2} b^2 + 2 \cdot n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} \cdot \frac{n-3}{4} a^{n-4} b^4 - \&c.}{(a^2 + b^2)^n};$$

a descending series will always converge, when x is greater than the greatest root of the resulting equations; and x^{n-1} , when n is infinite, is infinitely greater than $(a+b)^n$ and $(a-b)^n$; or more generally than $2a^n - 2n \cdot \frac{n-1}{2} a^{n-2} b^2 + 2n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} \cdot \frac{n-3}{4} a^{n-4} b^4 - \&c.$

This follows from Caput 3. of the *Meditationes Algebraicæ*.

Cor. It appears from hence, that, if the ascending series converges, the descending will diverge; and, *vice versa*, if the descending converges, the ascending will diverge, unless all the roots of the above-mentioned resulting equations may be deemed
of

of equal magnitude, as $+\alpha$ and $\alpha, \alpha\sqrt{-1}, \&c.$ and $x=\alpha$; in which case sometimes both series may become the same converging series, $\&c.$

When x , in the preceding cases, is equal to the least or greatest root, the series will sometimes converge, and sometimes not, as is shewn in the above-mentioned chapter. Whether the sum of a series, whose general term is given, can be found or not, will in many cases appear from the law of the multinomial and other more general series.

2. There are series which always converge, whatever may be the value of x ; as, for example, the series $1 + \frac{1}{2 \cdot 3}x + \frac{1}{2 \cdot 3 \cdot 4 \cdot 5}x^2 + \&c.$ or $1 + \frac{x}{2} + \frac{x^2}{3} + \frac{x^3}{4} + \&c.$ $\&c.$ always converge, whatever may be the value of x ; but it may be observed, that these series never arise from the expansion of algebraical functions of x , or the before-mentioned fluents; but, in a few cases, they may from fluxional equations. There are also series which never converge as $1 + 1 \cdot 2x + 1 \cdot 2 \cdot 3x^2 + 1 \cdot 2 \cdot 3 \cdot 4x^3 + \&c.$ to which the preceding remark may be applied.

3. In the year 1757 some papers, which contained the first edition of my *Meditationes Algebraicæ*, were sent to the Royal Society, in which was contained the following rule, *viz.* let S be a given function of the quantity x , which expand into a series $(a + bx^m + cx^{2m} + \&c.)$ proceeding according to the dimensions of x ; in the quantity S , for x^m write $\alpha x^m, \beta x^m, \gamma x^m, \&c.$ where $\alpha, \beta, \gamma, \&c.$ are roots of the equation $x^n - 1 = 0$; and let the resulting quantities be $A, B, C, D, \&c.$ then will $\frac{A+B+C+D+\&c.}{n}$ be equal to the sum of the first, $2n+1, 3n+1, \&c.$ terms *in infinitum*. This method, in the preface to the

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last edition of the *Meditationes Algebraicæ*, is rendered more correct and general.

4. Let the sum of a series required be expressed by a function of a quantity z , the distance from the first term of the series, then will the general term be the difference between the two successive sums generally expressed.

5. Let the general term be an algebraical function of z :

1st, let it be $\frac{az^m + bz^{m-1} + cz^{m-2} + \&c.}{z+e \cdot z+e+1 \cdot z+e+2 \dots z+e+n-1} = T$, where m and n are whole numbers; and m (if the sum of an infinite series of terms is required) less than n by two or more:

then the general term $\frac{az^m + bz^{m-1} + \&c.}{z+e \cdot z+e+1 \cdot z+e+2 \dots z+e+n-1} =$
 $\frac{\gamma}{z+e \cdot z+e+1} + \frac{\delta}{z+e \cdot z+e+1 \cdot z+e+2} +$
 $\frac{\epsilon}{z+e \cdot z+e+1 \cdot z+e+2 \cdot z+e+3} + \&c. \dots \frac{\theta}{z+e \cdot z+e+1 \dots z+e+n-1}$;

whence if

$z+e+2 \cdot z+e+3 \cdot z+e+4 \dots z+e+n-1 = z^{n-2} + Az^{n-3} + Bz^{n-4} + \&c.$;
 $z+e+3 \cdot z+e+4 \cdot z+e+5 \dots z+e+n-1 = z^{n-3} + A'z^{n-4} + B'z^{n-5} + \&c.$;
 $z+e+4 \cdot z+e+5 \dots z+e+n-1 = z^{n-4} + A''z^{n-5} + B''z^{n-6} + \&c.$ and so
 on; then, if $m = n - 2$, will $\gamma = a$, $\delta = b - \gamma A$, $\epsilon = c - \delta A' - \gamma B$,
 $\zeta = d - \epsilon A'' - \delta B' - \gamma C$, &c.; whence the integral *in infinitum*,
 or sum of the infinite series, will be $\frac{\gamma}{z+e} + \frac{\delta}{2 \cdot z+e \cdot z+e+1} +$

$\frac{\epsilon}{3 \cdot z+e \cdot z+e+1 \cdot z+e+2} + \&c.$

The reduction of the general term T into quantities of the before given formulæ was published in the *Meditationes*, printed in the year 1774. It was before reduced into formulæ of the same kind nearly by Mr. NICHOLE in the *Paris Act's*.

2d, Let the general term be $T' =$

$\frac{az^b + bz^{b-1} + cz^{b-2} + \&c.}{z+e \cdot z+e+1 \cdot z+e+2 \dots z+e+n-1 \times z+f \cdot z+f+1 \cdot z+f+m-1 \times z+g \cdot z+g+1 \dots z+g+l-1 \times \&c.}$

where

where b is a whole number less than $n+m+l+\&c.$ (if it be greater, then the fraction can easily be reduced into a rational quantity, $az^{b-n-m-l-\&c.} + \&c.$ and a fraction of the before-mentioned kind); then will $T' = \left(\frac{\alpha}{z+e} + \frac{\alpha'}{z+f} + \frac{\alpha''}{z+g} + \&c. \right) +$
 $\left(\frac{\beta}{z+e \cdot z+e+1} + \frac{\beta'}{z+f \cdot z+f+1} + \frac{\beta''}{z+g \cdot z+g+1} + \&c. \right) +$
 $\left(\frac{\gamma}{z+e \cdot z+e+1 \cdot z+e+2} + \frac{\gamma'}{z+f \cdot z+f+1 \cdot z+f+2} + \frac{\gamma''}{z+g \cdot z+g+1 \cdot z+g+2} \right.$
 $\left. + \&c. \right) \dots \left(\frac{X}{z+e \cdot z+e+1 \dots z+e+n-1} + \frac{X'}{z+f \cdot z+f+1 \dots z+f+m-1} \right.$
 $\left. + \frac{X''}{z+g \cdot z+g+1 \dots z+g+l-1} + \&c. \right)$; whence its integral *in infinitum*, that is, the sum of the infinite series can be found, when $\alpha=0$, $\alpha'=0$, $\alpha''=0$, $\&c.$; and consequently b not greater than $n+m+l+\&c.-2$; otherwise not. If b is not greater than $n+m+l+\&c.-2$, then will $\alpha+\alpha'+\alpha''+\&c.=0$, for else the sum would be infinite.

Let the number of quantities ($e, f, g, \&c.$) be r , then from r independent integrals of a series, whose term is T' ; or from $(r-1)$ independent sums of infinite serieses, whose term is T' ; that is, where b is not greater than $n+m+l+\&c.-2$; can be deduced the sum of all infinite serieses of the before-mentioned formulæ, whose general term is T' .

If any factors are deficient in the denominator, as suppose the term to be $z+e \times z+e+3 \times z+e+n-1$; multiply the numerator and denominator by the deficient factors, *viz.* by $z+e+1 \cdot z+e+2 \times z+e+4 \cdot z+e+5 \dots z+e+n-2$, and it acquires the preceding formula; and so in the following examples.

3d, Let the denominator be $\frac{x+e}{x+e+2} \times \frac{x+e}{x+e+1} \times \frac{x+e}{x+e+n-1} \times \frac{x+e}{x+e+\mu} \times \frac{x+e}{x+e+\mu+1} \times \dots$
 $\frac{x+e}{x+e+2} \dots \frac{x+e}{x+e+n-1} \times \frac{x+e}{x+e+\mu} \times \frac{x+e}{x+e+\mu+1} \times \dots$
 $\frac{x+e}{x+e}$

$\frac{x+e+\mu+2}{x+f+m-1} \times \&c. \times \frac{x+f^2}{x+f+1} \times \frac{x+f+2}{x+f+2} \dots \times$
 $\frac{x+f+m-1}{x+f+m-1} \times \&c. = D$, where $\pi, \pi', \rho, \&c.; \mu, \&c.$ are
 whole numbers; and the general term is $\frac{ax^b + bx^{b-1} + cx^{b-2} + \&c.}{D}$

$= T''$; then, if the dimensions of z in the numerator be
 less than its dimensions in the denominator, will $T'' =$

$$\left(\frac{\alpha}{z+e} + \frac{\alpha'}{(z+e)^2} + \frac{\alpha''}{(z+e)^3} \dots \frac{\alpha^{\pi+\pi'-1}}{(z+e)^{\pi+\pi'}} + \frac{\beta}{z+f} + \frac{\beta'}{(z+f)^2} + \frac{\beta''}{(z+f)^3} \dots \right.$$

$$\left. \frac{\beta^{\rho-1}}{(z+f)^{\rho}} + \&c. \right) + \left(\frac{\gamma}{z+e \cdot z+e+1} + \frac{\theta}{z+f \cdot z+f+1} + \&c. \right) + \&c.;$$

and in general there will be included all terms of the formulæ,

$$\frac{A(z+e+i^p - z+e^p)}{(z+e)^p \cdot (z+e+1)^p \dots (z+e+i)^p},$$

$$\frac{B((z+f+i')^p - (z+f)^p)}{(z+f)^p \cdot (z+f+1)^p \dots (z+f+i')^p}, \&c.$$

$$\frac{C((z+e+\mu+i'')^{p''} - (z+e+\mu)^{p''})}{(z+e+\mu)^{p''} \cdot (z+e+\mu+1)^{p''} \dots (z+e+\mu+i'')^{p''}}, \&c.$$

where $A, B, C, \&c. \alpha, \alpha', \&c. \beta, \beta', \&c. \gamma, \theta, \&c.$ denote in-
 variable quantities; and $p, p', p'', \&c.$ are whole numbers not
 greater than $\pi, \rho, \pi', \&c.$ respectively; and $i, i', i'', \&c.$ are
 whole numbers not greater than $n-1, m-1, \&c.$

If all the quantities $\alpha, \alpha', \alpha'', \&c. \beta, \beta', \beta'', \&c. \&c.$ are
 $= 0$, the sum of the series can be expressed in finite terms of
 the quantity z , otherwise not; and also if b be less than the
 dimensions of z in the denominator by two or more, then will
 $\alpha + \beta + \&c. = 0$, otherwise the sum would be infinite.

From $\pi + \pi' + \rho + \&c. = 1$ independent sums of infinite se-
 rieses of this kind can be deduced the sums of all infinite
 serieses of the same kind.

This method may be extended to infinite series, in which exponentials as e^x are contained, which will easily be seen from some subsequent propositions; but in my opinion the subsequent method of finding the sum of serieses is to be preferred to the preceding one, both for its generality and facility.

6. 1. Let the general term be $(ax^b + bx^{b-1} + cx^{b-2} + \&c.) \times (z+e)^{-1} \cdot (z+e+1)^{-1} \cdot (z+e+2)^{-1} \dots (z+e+n-1)^{-1}$; where b is a whole number less than n by two or more, when the sum of an infinite series is required.

Assume for the sum the quantity $(z+e)^{-1} \cdot (z+e+1)^{-1} \cdot (z+e+2)^{-1} \dots (z+e+n-2)^{-1} \times (\alpha z^{b'} + \beta z^{b'-1} + \gamma z^{b'-2} + \&c.)$; find the difference between this sum and its successive one $(z+e+1)^{-1} \cdot (z+e+2)^{-1} \cdot (z+e+3)^{-1} \dots (z+e+n-1)^{-1} \times (\alpha z^{b'} + \beta z^{b'-1} + \gamma z^{b'-2} + \&c.)$, which will be $-(z+e)^{-1} \cdot (z+e+1)^{-1} \cdot (z+e+2)^{-1} \dots (z+e+n-1)^{-1} \times (\alpha z^{b'} + \beta z^{b'-1} + \gamma z^{b'-2} + \&c.) - (z+e+1)^{-1} \cdot (z+e+2)^{-1} \dots (z+e+n-1)^{-1} \times (\alpha z^{b'} + \beta z^{b'-1} + \gamma z^{b'-2} + \&c.) - z+e+n-1 \times (\alpha z^{b'} + \beta z^{b'-1} + \gamma z^{b'-2} + \&c.) = \overline{b'-n+1} \alpha z^{b'} + \&c.$; then make the terms of this difference equal to the correspondent terms of the given quantity $ax^b + bx^{b-1} + \&c.$ and there result $b' = b$, $-\overline{b-n+1} \times \alpha = a$, and consequently $\alpha = \frac{-a}{b-n+1}$, $\&c.$

2. Let the general term be $(z+e)^{-1} \cdot (z+e+1)^{-1} \cdot (z+e+2)^{-1} \dots (z+e+n-1)^{-1} \times (z+f)^{-1} \cdot (z+f+1)^{-1} \cdot (z+f+2)^{-1} \dots (z+f+m-1)^{-1} \times (ax^b + bx^{b-1} + cx^{b-2} + \&c.)$. Assume the quantity $(z+e)^{-1} \cdot (z+e+1)^{-1} \dots (z+e+n-2)^{-1} \times (z+f)^{-1} \cdot (z+f+1)^{-1} \cdot (z+f+2)^{-1} \dots (z+f+m-2)^{-1} \times (\alpha z^{b'} + \beta z^{b'-1} + \gamma z^{b'-2} + \&c.)$ for the sum of the series sought; and thence deduce the general term, which suppose equal to the given general term, and from equating their corresponding parts easily can be deduced the index b' and co-efficients α , β , γ , $\&c.$ and consequently the sum of the series sought.

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3. Let the general term reduced to its lowest dimensions be

$$\frac{z+e}{z+e+1} \times \frac{z+e+1}{z+e+2} \dots \frac{z+e+n-1}{z+e+n} \times \frac{rz+f}{rz+f+r} \times \frac{rz+f+r}{rz+f+2r} \dots \frac{rz+f+m-1r}{rz+f+m} \times \frac{z+g}{z+g+1} \times \frac{z+g+1}{z+g+2} \dots \frac{z+g+l-1}{z+g+l} \times \&c. \times (az^b + bz^{b-1} + cz^{b-2} + \&c.).$$

If it be required to reduce the term $\frac{rz+f}{rz+f+r}$, &c. to a conformity with the rest, for $\frac{rz+f}{rz+f+r}$, &c. substitute $z + \frac{f}{r} \times r^{-e}$, &c. and it is done. Assume for the integral or sum the quantity

$$S = \frac{z+e}{z+e+1} \times \frac{z+e+1}{z+e+2} \dots \frac{z+e+n-2}{z+e+n-1} \times \frac{rz+f}{rz+f+r} \times \frac{rz+f+r}{rz+f+2r} \dots \frac{rz+f+m-2r}{rz+f+m-1r} \times \frac{z+g}{z+g+1} \times \frac{z+g+1}{z+g+2} \dots \frac{z+g+l-2}{z+g+l-1} \times \&c. \times (\alpha z^{b'} + \beta z^{b'-1} + \&c.) = S,$$

find its successive sum by writing $z+1$ for z in the sum S , and let the quantity resulting be S' ; then will the general term be $S - S'$, which equate to the given general term, that is, their correspondent quantities; and thence may be deduced the index b' and co-efficients α , β , &c.; and consequently the sum sought. If the series does not terminate, then the sum will be expressed by a series proceeding *in infinitum*, according to the reciprocal dimensions of z .

From $\pi + e + \sigma + \&c. - 1$ independent integrals of the above-mentioned kind can be deduced the integrals of all quantities of the same kind; that is, where b is any whole affirmative number whatever, and the co-efficients a , b , c , &c. are any how varied.

If any factor $z+g$ in the denominator, &c. has no other $z+g+l-1$, which differs from it by a whole number $l-1$; or the factor $rz+f$ has no correspondent factor $rz+f+mr$, where m is a whole number; then the integral of the above-mentioned series cannot be expressed in finite terms of the quantity z . In like manner, if the dimensions of z in the numerator are less than

than its dimensions in the denominator by unity, then the integral of the general term cannot be expressed by a finite algebraical function of x . If the number of terms to be added be infinite, it is well known that the sum in this case will be infinite.

It may be observed, that in finding the sum of a series, whose general term is given, all common divisors of the numerator and denominator must be rejected, otherwise serieses may appear difficult to be summed, which are very easy: for example, let the series be

$$\frac{1}{1 \cdot 2 \cdot 3 \cdot 4 \cdot 5} + \frac{4}{4 \cdot 5 \cdot 6 \cdot 7 \cdot 8} + \frac{9}{7 \cdot 8 \cdot 9 \cdot 10 \cdot 11} + \&c.$$

$$= \frac{1}{3} \left(\frac{1}{1 \cdot 2 \cdot 4 \cdot 5} + \frac{2}{4 \cdot 5 \cdot 7 \cdot 8} + \frac{3}{7 \cdot 8 \cdot 10 \cdot 11} + \&c. \right), \text{ whose}$$

$$\text{general term is } \frac{x+1}{3x+1 \cdot 3x+4 \cdot 3x+2 \cdot 3x+5}; \text{ and by assuming, as}$$

$$\text{as before taught, } \frac{1}{3x+1} \times \frac{1}{3x+2} \times \alpha \text{ for the sum sought;}$$

$$\text{and finding its general term } \frac{1}{3x+1} \times \frac{1}{3x+4} \times \frac{1}{3x+2} \times$$

$$\frac{1}{3x+5} \times 18 \frac{1}{x+1} \times \alpha, \text{ which equating to the general term}$$

$$\text{given, there results } 18\alpha = 1, \text{ and the sum sought} = \frac{1}{18} \times$$

$$\frac{1}{3x+1 \cdot 3x+2}.$$

$$\text{Ex. 2. Let the series be } \frac{1}{1 \cdot 2 \cdot 3 \cdot 4 \cdot 5} + \frac{14}{5 \cdot 6 \cdot 7 \cdot 8 \cdot 9} +$$

$$\frac{55}{9 \cdot 10 \cdot 11 \cdot 12 \cdot 13} + \frac{140}{13 \cdot 14 \cdot 15 \cdot 16 \cdot 17} + \&c. = \frac{1}{24} \left(\frac{1}{1 \cdot 5} + \frac{1}{5 \cdot 9} + \right.$$

$$\left. \frac{1}{9 \cdot 13} + \frac{1}{13 \cdot 17} + \&c. \right), \text{ of which the general term is } \frac{1}{24} \times$$

$$\frac{1}{4x+1 \cdot 4x+5}; \text{ and consequently the sum deduced is } \frac{1}{24} \times$$

$$\frac{1}{4} \times \frac{1}{4x+1}.$$

These are serieses given by Mr. DE MOIVRE, and esteemed by Dr. TAYLOR *altioris indaginis*.

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Some other writers have made some serieses to appear more difficult to be summed, by not reducing them to their lowest terms.

7. Having given the principles of a general method of finding the sum of a series, when its general term can be expressed by algebraical, and not exponential, functions of z , the distance from the first term of the series; it remains to perform the same when exponentials are included.

1. Let S the sum be any algebraical function of z multiplied into $e^z = x^z$; then will the general term be $Se^z - eS'e^z = (S - eS')e^z$; whence, from the general term Te^z being given, assume quantities in the same manner (with the same denominator, &c.) as when no exponential was involved, which multiplied into e^z , suppose to be the sum; from the sum find its general term, and equate it to the given one by equating their correspondent co-efficients, and it is done.

Ex. Let the general term be $\frac{z+2}{2z+1 \cdot 2z+3} \times e^{z+1}$: assume for the sum sought $\frac{\alpha}{2z+1} \times e^{z+1}$, whence the general term is $\left(\frac{\alpha}{2z+1} - \frac{\alpha e}{2z+3}\right) e^{z+1} = \frac{2\alpha(1-e)z + 3\alpha - \alpha e}{2z+1 \cdot 2z+3} \times e^{z+1}$; equate it to the given term, and there results $2\alpha(1-e) = 1$ and $3\alpha - \alpha e = 2$, and consequently $e = \frac{1}{3}$ and $\alpha = \frac{1}{2}$, if the series can be summed.

The same observation, viz. that if any factor in the denominator or irrational quantity have no other correspondent to it; for example, if the factor be $z+g$, and there is no correspondent one $x+g+n$, where n is a whole number, then its integral cannot be expressed by a finite algebraical function of z .

In the same manner may the sums be found, when the terms are exponentials of superior orders; for the exponential, irra-

tional, &c. quantities in the denominators of the sums may be easily deduced from the preceding principles; and thence, by proceeding as is before taught, the sum required.

The principles of all these cases have been given in the Meditations.

8. Mr. JAMES BERNOULLI found summable serieses by assuming a series V, whose terms at an infinite distance are infinitely little, and subtracting the series diminished by any number (l) of terms from the series itself, &c.

It is observed in the Meditations, that if $T(m)$, $T(m+n)$, $T(m+n+n')$, $T(m+n+n'+n'')$, &c. be the terms at m , $m+n$, $m+n+n'$, $m+n+n'+n''$, &c. distances from the first, and $aT(m) + bT(m+n) + cT(m+n+n') + dT(m+n+n'+n'') + \&c.$ be the general term, it will be summable, when $a+b+c+d+\&c. = 0$; the sum of the series will be $a(T(m) + T(m+1) + T(m+2) + \dots + T(m+n+n'+n''+\&c.-1)) + b(T(m+n) + T(m+n+1) + T(m+n+2) + \dots + T(m+n+n'+n''+\&c.-1)) + c(T(m+n+n') + T(m+n+n'+1) + \dots + T(m+n+n'+n''+\&c.-1)) + \&c. = H$. If the sum $a+b+c+d+\&c.$ be not $= 0$, and the series $T(m) + T(m+1) + T(m+2) + \&c.$ in infinitum be a converging one $= S$, then will the sum of the resulting series be $(a+b+c+d+\&c.) S - (b+c+d+\&c.) (T^m \dots + T^{m+n-1}) - (c+d+\&c.) (T^{m+n} \dots T^{m+n+n'-1}) - (d+\&c.) (T^{m+n+n'} + \dots T^{m+n+n'+n''-1}) - \&c.$

8. 2. Let the series V consist of terms, which have only one factor in the denominator, and its numerator $= 1$; that is, let the general term be $\frac{1}{rx+e}$, and the series consequently

$\frac{1}{e} + \frac{1}{r+e} + \frac{1}{2r+e} + \&c. = V$; from the before-mentioned addition

or subtraction there follows $\frac{a}{rx+e} + \frac{b}{rx+r+e} + \frac{c}{rx+2r+e} + \&c. =$

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$ax^m +$

$\frac{\alpha z^m + \beta z^{m-1} + \gamma z^{m-2} + \&c.}{rz + e \cdot rz + r + e \cdot rz + 2r + e \cdot \&c.}$; where m is not greater than the number (N) of factors in the denominator diminished by unity. From $\alpha, \beta, \gamma, \&c.$ r and e being given, easily can be acquired by simple equations, or known theorems, the required co-efficients $a, b, c, \&c.$ If $m = N - 1$ and α and $a + b + c + d + \&c. = 0$, then the sum of the series resulting will be finite.

8. 3. If the terms of the series assumed $\frac{1}{e} - \frac{1}{r+e} + \frac{1}{2r+e} - \frac{1}{3r+e} + \&c.$ be alternately affirmative and negative; then by the preceding case find $\frac{\alpha z^m + \beta z^{m-1} + \gamma z^{m-2} + \&c.}{rz + e \cdot rz + r + e \cdot rz + 2r + e + \&c.} =$

$\frac{a}{rz+e} + \frac{b}{rz+r+e} + \frac{c}{rz+2r+e} + \&c.$ Where the terms of the resulting series are alternately affirmative and negative, let the

two subsequent terms be supposed: $\frac{\alpha z^m + \beta z^{m-1} + \gamma z^{m-2} + \&c.}{rz + e \cdot rz + r + e \cdot rz + 2r + e \cdot rz + n - 1r + e}$

$= \frac{a}{rz+e} + \frac{b}{rz+r+e} + \&c.$ and $\frac{\alpha z^{m-1} + \beta z^{m-2} + \gamma z^{m-3} + \&c.}{rz + r + e \cdot rz + 2r + e \cdot rz + nr + e} =$

$\frac{a}{rz+r+e} + \frac{b}{rz+2r+e} + \&c.$ of which the one is affirmative and the

other negative: reduce the resulting series to an affirmative one by subtracting the subsequent term from its preceding, and it becomes

$\frac{(rz + nr + e)(\alpha z^m + \beta z^{m-1} + \&c.) - (rz + e)(\alpha z^{m-1} + \beta z^{m-2} + \&c.)}{rz + e \cdot rz + r + e \cdot rz + 2r + e \cdot rz + nr + e}$

$= \frac{n - mra z^m + \&c.}{rz + e \cdot rz + r + e \cdot rz + nr + e} = \frac{a}{rz+e} + \frac{b-a}{rz+r+e} + \&c.$ In this

case, since two terms are added into one, the distance from the first term of the series will be $\frac{z}{2}$, which suppose $= w$; and write

$2w$ for z in the above-mentioned term, and there results

$\frac{n - mra z^m + \&c.}{rz + e \cdot rz + r + e \cdot rz + nr + e} = \frac{n - mra \times 2^m w^m + \&c.}{2rw + e \cdot 2rw + r + e \cdot 2rw + nr + e} =$
 $\frac{a}{2rw + e}$

$\frac{a}{2rw+e} + \frac{b-a}{2rw+r+e} + \&c.$; whence the sum of any series, whose general term is $\frac{a'w^m + b'w^{m-1} + \&c.}{2rw+e \cdot 2rw+r+e \dots 2rw+nr+e}$, where m is a whole number less than n by two or more, and w the distance from the first term of the series can be found from the sum of the series $\frac{1}{a} - \frac{1}{r+e} + \frac{1}{2r+e} - \frac{1}{3r+e} + \&c.$

9. Let there be two serieses $\frac{1}{e} + \frac{1}{e+r} + \frac{1}{e+2r} + \&c. = S$ and $\frac{1}{f} + \frac{1}{f+r} + \frac{1}{f+2r} + \frac{1}{f+3r} + \&c. = S'$, whose general terms are respectively $+\frac{1}{e+rz}$ and $+\frac{1}{f+rz}$; then from the sum of these two serieses can be collected the sum of any series, whose general term is

$$\frac{\alpha z^m + \beta z^{m-1} + \&c.}{rz+e \cdot rz+e+r \cdot rz+e+2r \dots rz+n-1r + e \cdot rz+f \cdot rz+r+f \dots rz+f+m-1r} \\ = \frac{a}{rz+e} + \frac{b}{rz+e+r} + \frac{c}{rz+e+2r} \dots + \frac{\lambda}{rz+n-1r+e} + \frac{a'}{rz+f} + \frac{b'}{rz+r+f} \\ + \frac{c'}{rz+2r+f} \dots + \frac{\mu'}{rz+m-1r+f}; \text{ where } e-f \text{ is not a whole number. Let } a+b+c \dots +\lambda=0, \text{ and } a'+b'+c' \dots +\mu'=0, \text{ then the sum will be } a \left(\frac{1}{rz+e} + \frac{1}{rz+r+e} \dots + \frac{1}{rz+n-2r+e} \right) + b \left(\frac{1}{rz+e+r} + \frac{1}{rz+e+2r} \dots + \frac{1}{rz+n-2r+e} \right) + c \left(\frac{1}{rz+e+2r} + \frac{1}{rz+e+3r} + \dots + \frac{1}{rz+n-2r+e} \right) + \&c. + a' \left(\frac{1}{rz+f} + \frac{1}{rz+r+f} \dots + \frac{1}{rz+m-2r+f} \right) + b' \left(\frac{1}{rz+r+f} + \dots + \frac{1}{rz+m-2r+f} \right) + \&c.$$

2. If the serieses are $\frac{1}{e} - \frac{1}{e+r} + \frac{1}{e+2r} - \&c.$ and $\frac{1}{f} - \frac{1}{f+r} + \frac{1}{f+2r} - \&c.$; then from the sum of these two serieses can be collected by the principles given above the sum of any series, whose general term is

$$\alpha z^m + \beta z^{m-1} + \gamma z^{m-2} + \&c.$$

$$\frac{\alpha z^m + \beta z^{m-1} + \gamma z^{m-2} + \&c.}{2rz + e. 2rz + r + e. 2rz + 2r + e \dots 2rz + n - 1r + e \times 2rz + f. 2rz + r + f. 2rz + 2r + f \dots 2rz + m - 1r + f}$$

The same principle may be applied to find the sum of any series of the abovementioned sort, in whose denominator are contained other factors, $rz + g$, $rz + g + r$, &c. &c.; or $2rz + g$, $2rz + g + r$, $2rz + g + 2r$, &c. Like propositions may be deduced from serieses, in which r and r' , &c. and the factors $rz + e$ and $r'z + g$, &c. denote different quantities.

10. An apparently more general method may be given from assuming a series or serieses as before; and adding every two, three, four, &c. (n) successive terms together for terms of a new series beginning from the first, second, third, &c. n^{th} term; and in general adding together two, three, &c. n successive general terms; and in their sum writing for z the distance from the first term of the series $2z + a$, $3z + a$, &c. $nz + a$; there will result the general term of a series not to be found from the above-mentioned addition.

Ex. Let the series assumed be $\frac{1}{z} + \frac{1}{z+1} + \frac{1}{z+2} + \frac{1}{z+3} + \&c.$ in infinitum, of which the general term beginning from the first is $\frac{1}{z+1}$; add three successive general terms $\frac{1}{z+1} + \frac{1}{z+2} + \frac{1}{z+3} = \frac{3z^2 + 12z + 11}{z+1 \cdot z+2 \cdot z+3}$; in this term for z write $3z$, and there results $\frac{27z^2 + 36z + 11}{3z+1 \cdot 3z+2 \cdot 3z+3}$. In the same manner, if the beginning is instituted from the second or third term of the given series, the terms resulting will be $\frac{3z^2 + 18z + 26}{z+2 \cdot z+3 \cdot z+4}$ and $\frac{3z^2 + 24z + 47}{z+3 \cdot z+4 \cdot z+5}$. In these terms for z write $3z$, and there result $\frac{27z^2 + 54z + 26}{3z+2 \cdot 3z+3 \cdot 3z+4}$ and $\frac{27z^2 + 72z + 47}{3z+3 \cdot 3z+4 \cdot 3z+5}$.

If

If the terms of the given series are alternately affirmative and negative, the terms of the resulting series will be alternately affirmative and negative, if n be an odd number; otherwise its terms will be all affirmative. The sum of this series will be finite or infinite, as the sum of the series $1 + \frac{1}{2} + \frac{1}{3} + \frac{1}{4} + \&c.$ is finite or infinite; but from it, by the preceding method of addition or subtraction of Mr. BERNOULLI's, or a like method applied to more serieses, may be found the sums of different finite serieses.

It may be observed, that from Mr. BERNOULLI's addition or subtraction can never be deduced the serieses which arise from this method; for, by his method, the denominator can never have any factors but what are contained in the denominators of the given series, viz. (in the series $\frac{1}{1} + \frac{1}{2} + \frac{1}{3} + \&c.$), $z + l$, where l is a whole number; but by this method are introduced into the denominator the factors $2z + l$, $3z + l$, $\&c.$ and $nz + l$, or which may be reduced to the same $(z + \frac{l}{n}) \times n$.

If n successive general terms of the serieses arising from Mr. BERNOULLI's addition or subtraction be added together, and in the quantity thence arising for z the distance from the first term of the series be substituted nz , there will be produced serieses of the above-mentioned formula.

11. Multiply two converging serieses $a + bx + cx^2 + dx^3 + \&c. = S$ and $\alpha + \beta x + \gamma x^2 + \&c. = V$, or find any rational and integral function of them, and the series resulting will be finite and $= S \times V$, $\&c.$ Let $\alpha + \beta x + \gamma x^2 + \&c. x^n = V$ be finite, and the resulting series will be finite and $= S \times V$, $\&c.$ If S be a series converging or not, whose ultimate terms are less than any finite quantity, then will the series $(a + bx + cx^2 + \&c.) \times (\alpha + \beta x + \gamma x^2 + \&c. x^n) = V \times S$ be a converging one, if $\alpha + \beta x + \gamma x^2 + \dots \&c. x^n = 0$; which case was given by Mr. DE MOIVRE.

Mr.

Mr. BERNOULLI's addition, &c. can be applied to serieses of this kind. For example, let the given series be $\frac{1}{e} + \frac{1}{e+1}x + \frac{1}{e+2}x^2 + \&c. = S$. From this series subtract the same series diminished by m terms, viz. $\frac{1}{e+m}x^m + \frac{1}{e+m+1}x^{m+1} + \frac{1}{e+m+2}x^{m+2} + \&c.$ and there remains $\frac{e+m-ex^m}{e \cdot m+e} + \frac{e+m+1-e+1x^m}{e+1 \cdot e+m+1}x + \frac{e+m+2-e+2x^m}{e+2 \cdot e+m+2}x^2 + \frac{e+m+3-e+3x^m}{e+3 \cdot e+m+3}x^3 + \&c.$; for x^m write A , then will the series become $\frac{m-eA}{e \cdot m+e} + \frac{e+m+1-e+1A}{e+1 \cdot e+m+1}x + \frac{e+m+2-e+2A}{e+2 \cdot e+m+2}x^2 + \frac{e+m+3-e+3A}{e+3 \cdot e+m+3}x^3 + \&c. = \frac{1}{e} + \frac{1}{e+1}x + \frac{1}{e+2}x^2 \dots \dots \frac{1}{e+m-1}x^{m-1}$.

Let the general term be $\frac{ax^m + bx^{m-1} + cx^{m-2} + \&c.}{z+e \cdot z+e+1 \cdot z+e+2 \dots z+e+n-1} \times x^n$
 $= \left(\frac{a}{z+e} + \frac{\beta}{z+e+1} + \frac{\gamma}{z+e+2} \dots \frac{X}{z+e+n-1} \right) x^n$. Suppose $\beta = \beta'x$, $\gamma = \gamma'x^2$, $\delta = \delta'x^3$, $\dots X = X'x^{n-1}$; then will the sum of the above-mentioned series be $(a + \beta' + \gamma' + \delta' + \&c.) \times S - \frac{1}{e}(\beta' + \gamma' + \delta' + \&c.) - \frac{1}{e+1}(\gamma' + \delta' + \&c.) - \frac{1}{e+2}(\delta' + \&c.) - \&c.$

From the sum of the series $\frac{1}{e} - \frac{x}{e+1} + \frac{x^2}{e+2} - \&c.$ by these and the principles before delivered can be deduced the sum of any series, whose general term is

$$\frac{ax^m + bx^{m-1} + \&c.}{2z+e \cdot 2z+e+1 \cdot 2z+e+2 \cdot 2z+e+3 \times \&c.} x^n.$$

In like manner from the sum of the serieses $\frac{x}{e} + \frac{x^2}{e+1} + \frac{x^3}{e+2} + \&c.$ $\frac{x}{f} + \frac{x^2}{f+1} + \frac{x^3}{f+2} + \&c.$ $\frac{x}{g} + \frac{x^2}{g+1} + \frac{x^3}{g+2} + \&c.$ &c. can be deduced

deduced the sum of any series, whose general term is

$$\frac{az^m + bz^{m-1} + \&c.}{z + e . z + e + 1 . z + e + 2 \&c. \times z + f . z + f + 1 . z + f + 2 . \&c. \times z + g . z + g + 1 . \&c.} \times x^m.$$

And also from the sum of the serieses $\frac{1}{e} - \frac{1}{e+1}x + \frac{x^2}{e+2} - \&c.$
 $\frac{1}{f} - \frac{x}{f+1} + \frac{x^2}{f+2} - \&c.$ $\frac{1}{g} - \frac{x}{g+1} + \frac{x^2}{g+2} - \&c.$ $\&c.$ can be deduced
 the sum of any series, whose general term is

$$\frac{az^m + bz^{m-1} + \&c.}{2z + e . 2z + e + 1 . \&c. \times 2z + f . 2z + f + 1 . \&c. 2z + g . 2z + g + 1 . \&c.} \times x^m.$$

The method of adding more terms of a given series together, as before taught, may be applied to these and all other serieses. For example: let the given series be $1 + \frac{1}{2}x + \frac{1}{3}x^2 + \frac{1}{4}x^3 + \&c.$; add two terms constantly together, and it becomes

$$1 + \frac{1}{2}x + \&c. = \frac{2+x}{2} + \frac{4+3x}{3 \cdot 4}x^2 + \frac{6+5x}{5 \cdot 6}x^3 + \&c. = \frac{2+A}{2} + \frac{4+3A}{3 \cdot 4}x^2 + \frac{6+5A}{5 \cdot 6}x^3 + \&c. \text{ whence the general term is } \frac{2z+2+(2z+1)}{2z+2}$$

$\frac{x}{2z+1}x^{2z}$. From the methods before given of addition, subtraction, and multiplication; and the serieses found by this method, can be derived serieses, whose sums are known.

12. Suppose a given series $ax^n + bx^{n\pm 1} + cx^{n\pm 2} + dx^{n\pm 3} + \&c.$ whose sum p is either an algebraical, exponential, or fluent fluxion of x ; multiply the equation $p = ax^n + bx^{n\pm 1} + cx^{n\pm 2} + dx^{n\pm 3} + \&c.$ into $x^{\pm r-n}$, and there results $ax^{\pm r} + bx^{\pm r\pm 1} + cx^{\pm r\pm 2} + \&c.$; find the fluxion of this equation, and there follows $\frac{1}{x}$ multiplied into the fluxion of the quantity $(x^{\pm r-n}p)$
 $= \pm rax^{\pm r-1} + (\pm r \pm s)bx^{\pm r\pm 1-1} + (\pm r \pm 2s)cx^{\pm r\pm 2-1} + \&c.$
 of which the general term is $(\pm r \pm zs) \times t$, where z denotes the distance from the first term of the series, and t

is the term in the given series, whose distance from the first is z . In the same manner may be deduced the sum of a series whose general term is $t' \times \pm r \pm zs \times \pm r' \pm z \pm ns$, or by repeated operations $t' \times \pm rz^2 + fz + g$, where t' is a term of the given equation, whose distance from the first term is z . And in general, from the sum of a given series, whose fluxion can be found, and whose general term is t' , can be deduced by continued multiplication, and finding the fluxion, the sum of a series or quantity, of which the general term is At' , where A is any function of the following kind $a'z^m + b'z^{m-1} + c'z^{m-2} + \&c.$ in which z denotes the distance from the first term of the series, and m a whole number. It is to be observed, that if the given series converges in a ratio, which is at least equal to the ratio of the convergency of some geometrical series, the resulting equation will always converge. But if in a less ratio, then it will sometimes converge, sometimes not, according to the ratio which the successive terms of the resulting series have to each other at an infinite distance.

Corollary. $\frac{p \cdot p + 1 \cdot p + 2 \cdot p + 3 \cdot p + z}{r \cdot r + 1 \cdot r + 2 \cdot r + 3 \cdot r + z} =$
 $\frac{p+z \cdot p+z-1 \cdot p+z-2 \cdot p+z-3 \cdot z+r+1}{r \cdot r+1 \cdot r+2 \cdot r+3 \cdot r+1}$, if $p-r$ be a whole affirmative number; but this latter quantity has the formula above-mentioned $az^m + bz^{m-1} + cz^{m-2} + \&c.$; and consequently, if the sum of the series $a + bx + cx^2 + dx^3 + \&c. = p$ be known, by this method can be deduced the sum of the series $a + \frac{p}{r}bx + \frac{p \cdot p + 1}{r \cdot r + 1}cx^2 + \frac{p \cdot p + 1 \cdot p + 2}{r \cdot r + 1 \cdot r + 2}dx^3 + \&c.$

Ex. 1. Since $\frac{m}{a+x^n} = a^{\frac{m}{n}} \left(1 + \frac{m}{n} \times \frac{x}{a} + \frac{m}{n} \times \frac{m-n}{2n} a^{-1}x^2 + \frac{m}{n} \cdot \frac{m-n}{2n} \cdot \frac{m-2n}{3n} a^{-2}x^3 + \&c. \right)$; multiply the successive terms of this series into

into the terms of the series $1, \frac{p}{r}, \frac{p \cdot p+1}{r \cdot r+1}, \&c.$ and a series is deduced $a^{\frac{m}{n}} + \frac{p \cdot m}{r \cdot n} a^{+\frac{m}{n}-1} x + \frac{p \cdot p+1 \times m \cdot m-n}{r \cdot r+1 \cdot n \cdot 2n} x^{+\frac{m}{n}-2} + \&c.$ whose sum is known, if the sum of the series $= a + x^{+\frac{m}{n}}$ is known.

Ex. 2. If the series begins from the $l + 1^{\text{th}}$ term of the above-mentioned binomial theorem $a^{\frac{m}{n}} + \frac{m}{n} a^{+\frac{m}{n}-1} x + \&c.$ viz. the series be $H \times 1 + \frac{m-l+1n}{l+2n} \frac{x}{a} + \frac{m-l+2n}{l+3n} \frac{x^2}{a^2} + \frac{m-l+3n}{l+4n} \frac{x^3}{a^3} + \&c.$ of which let the respective terms be multiplied into $1, \frac{p}{r}, \frac{p \cdot p+1}{r \cdot r+1}, \&c.$ there will result a series whose sum is known.

Ex. 3. From the rule first given by me for finding the sum of the terms at b distances from each other, the sum of the series $1 + \frac{m-l+1n}{l+2 \cdot n} \times \frac{m-l+2n}{l+3 \cdot n} \dots \frac{m-l+bn}{l+b+1n} \times \frac{x^b}{a^b} + P \times \frac{m-l+b+1n}{l+b+2n} \times \frac{m-l+b+2n}{l+b+3n} \dots \frac{m-l+2bn}{l+2b+1n} \frac{x^{2b}}{a^{2b}} + \&c.$ where P denotes the co-efficient of the preceding term, can be deduced; and consequently the sum of the series deduced from multiplying the successive terms of this series into the quantities $1, \frac{p}{r}, \frac{p \cdot p+1}{r \cdot r+1}, \&c.$ respectively.

The general principles of this case were first delivered by Mr. BERNOULLI, Mr. DE MOIVRE, Mr. EULER, &c.

12. Assume the series $a + bx^n + cx^{2n} + \&c. = p$, multiply it into $x^{n-1}x$, and find the fluent, then will $\frac{1}{a} x^n p - \frac{1}{a} \int x^n p = \frac{1}{a} a x^n +$

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$\frac{1}{\alpha+n} bx^{\alpha+n} + \frac{1}{\alpha+2n} cx^{\alpha+2n} + \&c.$; multiply this equation into $x^{\beta-\alpha-1}\dot{x}$, and find the fluent of the equation resulting, which will be $\frac{1}{\beta} \times \frac{1}{\alpha} x^{\beta} \dot{p} - \frac{1}{\alpha} \cdot \frac{1}{\beta} \int x^{\beta} \dot{p} - \frac{1}{\alpha} \times \frac{1}{\beta-\alpha} x^{\beta-\alpha} \int x^{\alpha} \dot{p} + \frac{1}{\alpha} \cdot \frac{1}{\beta-\alpha} \int x^{\beta} \dot{p} = \frac{1}{\alpha} \cdot \frac{1}{\beta} ax^{\beta} + \frac{1}{\alpha+n} \cdot \frac{1}{\beta+n} bx^{\beta+n} + \frac{1}{\alpha+2n} \cdot \frac{1}{\beta+2n} cx^{\beta+2n} + \&c.$; divide by x^{β} , and there results $\frac{1}{\beta} \cdot \frac{1}{\alpha} \dot{p} + \frac{1}{\alpha} \cdot \frac{1}{\alpha-\beta} x^{-\alpha} \int x^{\alpha} \dot{p} + \frac{1}{\beta} \cdot \frac{1}{\beta-\alpha} x^{-\beta} \int x^{\beta} \dot{p} = \frac{1}{\alpha} \cdot \frac{1}{\beta} a + \frac{1}{\alpha+n} \cdot \frac{1}{\beta+n} bx^n + \&c.$; and in general $\frac{1}{\alpha} \cdot \frac{1}{\beta} \cdot \frac{1}{\gamma} \cdot \&c. \dot{p} + \frac{1}{\alpha} \cdot \frac{1}{\alpha-\beta} \cdot \frac{1}{\alpha-\gamma} x^{-\alpha} \int x^{\alpha} \dot{p} + \frac{1}{\beta} \cdot \frac{1}{\beta-\alpha} \cdot \frac{1}{\beta-\gamma} \int x^{\beta} \dot{p} + \frac{1}{\gamma} \cdot \frac{1}{\gamma-\alpha} \cdot \frac{1}{\gamma-\beta} \int x^{\gamma} \dot{p} = \frac{1}{\alpha} \cdot \frac{1}{\beta} \cdot \frac{1}{\gamma} \&c. a + \frac{1}{\alpha+n} \cdot \frac{1}{\beta+n} \cdot \frac{1}{\gamma+n} \&c. bx^n + \frac{1}{\alpha+2n} \cdot \frac{1}{\beta+2n} \cdot \frac{1}{\gamma+2n} \&c. cx^{2n} + \&c.$ whence the law of continuation is immediately manifest.

Hence, if no two quantities $\alpha, \beta, \gamma, \delta, \&c.$ be equal to each other; and the successive terms $a, b, c, d, \&c.$ of any series $a + bx^n + cx^{2n} + \&c. = \dot{p}$ be divided by $\alpha.\beta.\gamma.\delta.\&c.$; $\frac{1}{\alpha+n} \cdot \frac{1}{\beta+n} \cdot \frac{1}{\gamma+n} \cdot \frac{1}{\delta+n} \cdot \&c.$; $\alpha+2n \cdot \beta+2n \cdot \gamma+2n \cdot \delta+2n \cdot \&c. \&c.$; and in general by $\alpha+nz \cdot \beta+nz \cdot \gamma+nz \cdot \delta+nz \cdot \&c. \&c.$; then can the sum of the series be found from the fluents of the fluxions $x^{\alpha}\dot{p}, x^{\beta}\dot{p}, x^{\gamma}\dot{p}, x^{\delta}\dot{p}, \&c.$ as has been observed in the Meditations. If two are equal, *viz.* $\alpha=\beta$, then also the fluent of the fluxion $\frac{\dot{x}}{x} \int x^{\alpha} \dot{p}$ is required. If three are equal *viz.* $\alpha=\beta=\gamma$; then it is necessary to find the fluent of the fluxion $\frac{\dot{x}}{x} \int \frac{\dot{x}}{x} \int x^{\alpha} \dot{p}$; and so on.

1. Let $\dot{p} = \frac{\dot{x}}{1 \pm x^n}$; and if the differences of the quantities $\alpha, \beta, \gamma, \delta, \&c.$ are divisible by n , from the fluent of the fluxion

fluxion $x^a \dot{p}$ can be deduced the fluents of all the other fluxions $x^b \dot{p}$, $x^c \dot{p}$, &c.; and in general, if $\alpha - \beta$ is divisible by n , then from the fluent of the fluxion $x^a \dot{p}$ can be deduced the fluent of the fluxion $x^b \dot{p}$.

2. Suppose p = the terms of the binomial theorem expanded according to the dimensions of x , viz. $(a + bx^n)^{\frac{r}{s}} = a^{\frac{r}{s}} + \frac{r}{s} a^{\frac{r}{s}-1} bx^n + \&c.$ beginning from the first or any other terms; then, if α , β , &c. divided by n give whole affirmative numbers, will all the fluxions $x^a \dot{p}$, $x^b \dot{p}$, $x^c \dot{p}$, &c. be integrable; and if the differences of the quantities α , β , γ , δ , &c. are divisible by n , from the fluent of the fluxion $x^a \dot{p}$ can be deduced the fluents of the fluxions $x^b \dot{p}$, $x^c \dot{p}$, &c.

If p denotes the sum of the alternate or terms whose distance from each other are m , of the binomial theorem, the same may be applied.

3. If $p = \overline{a + bx^n + cx^{2n} + \&c.}$; and α , β , γ , δ , &c. divided by n give whole affirmative numbers, then from $\int x^a \dot{p}$ can be deduced all the remainder $\int x^b \dot{p}$, $\int x^c \dot{p}$, &c.: and in general from two can be deduced all the remainder.

To find when the sum of any series of this kind can be found, add together each of the fluents, which can be found from each other, and not otherwise, and suppose their sum = 0; and so of any other similar fluent, and from the resulting equations can be discovered when the series can be integrated.

13. If the general term of a series contains in it more variable quantities, z , v , w , &c.; then find the sum of the series, first, from the hypothesis that one of them (z) is only variable,

ble, which, properly corrected, let be A ; in the quantity A suppose all the quantities invariable but some other v , and find the sum of the series thence resulting, which let be B , and so on; and the sum of the series will be deduced.

Ex. Let the term be $\frac{1}{z \cdot z+1 \times v \cdot v+1 \cdot v+2}$; the dimensions of z and v , &c. in the denominator must be at least greater than its dimensions in the numerator by a quantity greater than the number of the quantities z , v , &c. which proceed *in infinitum* increased by unity. First, suppose z only variable, and the sum of the infinite series resulting will be $\frac{1}{z \cdot v \cdot v+1 \cdot v+2} = A$; then suppose v only variable, and the sum resulting will be $\frac{1}{2z \cdot v \cdot v+1} = B$, which is the sum required.

If it be supposed, that the quantities z and v , &c. in the same term shall never have the same values, then suppose z and v always to have the same values, and the general term $\frac{1}{z \cdot z+1 \cdot v \cdot v+1 \cdot v+2}$ becomes $\frac{1}{z^2 \cdot z+1^2 \cdot z+2}$, of which let the sum be V , then will $B - V$ be the sum required.

On this and some other subjects more have been given in the *Meditationes*.

14. If the sum of the series cannot be found in finite terms, and it is necessary to recur to infinite series; it is observed in the *Meditationes* to be generally necessary to add so many terms together, that the distance from the first term of the series may considerably exceed the greatest root of an equation resulting from the general term made ± 0 ; and afterwards a series more converging may commonly be deduced from the fluents of fluxions resulting from neglecting all but the greatest quantities in the general terms resulting; and by other

different methods. Mr. NICHOLAS BERNOULLI and Mr. MOMMORT investigated the sum of the series (P) $A + Br + Cr^2 + \&c.$ by a series (Q) $\frac{A}{1-r} + \frac{d'r}{(1-r)^2} + \frac{d''r^2}{(1-r)^3} + \frac{d'''r^3}{(1-r)^4} + \&c.$; where d', d'', d''' , &c. denote the successive differences of the terms A, B, C, D, &c. If r be negative, the denominators become $1+r, (1+r)^2, (1+r)^3$, &c.

It has been observed, in the *Meditationes*, that in swift converging series the series P will converge more swiftly than the series Q; in series converging according to a geometrical ratio, sometimes the one will converge more swift, and sometimes the other. In other series, which converge more slow, where most commonly r nearly $=1$, it cannot in general be said, which of the serieses will converge the swiftest. The preceding remark, *viz.* the addition of the first terms of the series, is necessary in most cases of finding the sums by serieses of this kind.

It is not unworthy of observation, that in almost all cases of infinite series, the convergency depends on the roots of the given equations, which remark was first published in the *Meditationes*. For example: in finding approximates to the roots of given equations the convergency depends on how much the approximates given are more near to one root than to any other; and consequently, when two or more roots or values of an unknown quantity are nearly equal, different rules are to be applied, which are improvements of the rule of false. This rule, and the above-mentioned observations were first given in the *Meditationes Algebraicæ et Analyticæ*, with several other additions on similar subjects.

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Many more things concerning the summation of series, which depend on fluxional, &c. equations, might be added; but I shall conclude this paper with congratulating myself, that some algebraical inventions published by me have been since thought not unworthy of being published by some of the greatest mathematicians of this or any other age.

1st, In the year 1757, I sent to the Royal Society the first edition of my *Meditationes Algebraicæ*: they were printed and published in the years 1760 and 1762, with *Properties of Curve Lines*, under the title of *Miscellanea Analytica*, and a copy of them sent to Mr. EULER in the beginning of the year 1763, in which was contained a resolution of algebraical equations, not inferior, on account of its generality and facility, to any yet published (*viz.* $y = a \sqrt[p]{p} + b \sqrt[p]{p^2} + c \sqrt[p]{p^3} + \dots \sqrt[p]{p^{n-1}}$). This resolution was published by Mr. EULER in the *Petersburg Acts* for the year 1764. Whether Mr. EULER ever received my book, I cannot pretend to say; nor is it material: for the fact is, that it was published by me in the year 1760 and 1762, and first by Mr. EULER in the year 1764. Mr. DE LA GRANGE and Mr. BEZOUT have ascribed this resolution to Mr. EULER, as first published in the year 1764, not having seen (I suppose) my *Miscell. Analyt.* Mr. BEZOUT found from it some new equations, of which the resolution is known, and applied it to the reduction of equations: more new equations are given, and the resolution rendered more easy by me in the *Philosophical Transactions*. 2d, In the above-mentioned *Miscell. Analyt.* an equation is transformed into another, of which the roots are the squares of the differences of the roots of the given equation; and it is asserted in that book, that if the co-efficients

co-efficients of the terms of the resulting equations change continually from + to - and - to +, the roots of the given equation are all possible, otherwise not; and in a paper, inserted by me in the Philosophical Transactions for the year 1764, in which is found from this transformation, when there are none, two or four impossible roots contained in an algebraical equation of four or five dimensions; it is observed, that there will be none or four, &c. impossible roots contained in the given equation, if the last term be + or -; and two, &c. on the contrary, if the last term be - or +. These observations and transformation have been since published and explained in the Berlin Acts for the years 1767 and 1768, by Mr. DE LA GRANGE. 3d. In the Miscell. Anal. an equation is transformed into another, whose roots are the squares, &c. of the roots of a given equation; and it is asserted, that there are at least so many impossible roots contained in the given equation, as there are continual progresses in the resulting equation from + to + and - to -. It is afterwards remarked, that these rules sometimes find impossible roots when Sir ISAAC NEWTON's, and such like rules, fail; and that Sir ISAAC NEWTON's, &c. will find them, when this rule fails. This rule may somewhat further be promoted by first changing the given equation, whose root is x , into another whose root is $\sqrt{-1}x$; but, in my opinion, the rule of HARRIOT's, which only finds whether there are impossible roots contained in a cubic equation or not, is to be preferred to these rules, which, in equations of any dimensions, of which the impossible roots cannot generally be found from the rules, seldom find the true number. 4th, It is remarked, that rules which discover the true number of impossible roots require immense calculations, since they must necessarily find, when

the roots become equal. In order to this, in the *Miscell. Anal.* there is found an equation, whose roots are the reciprocals of the differences of any two roots of the given equation; and from finding a quantity (π) greater than the greatest root of the given, and $\left(\frac{1}{A}\right)$ greater than the greatest root of the resulting equation, and substituting π , $\pi - A$, $\pi - 2A$, &c. for x in the given equation; will always be found the true number of impossible roots. 5th, In the same book are assumed two equations $(nx^{n-1} - n - 1px^{n-2} + n - 2qx^{n-3} - \&c. = 0$ and $x^n - px^{n-1} + \&c. = w)$, and thence deduced an equation, whose root is w , from which, in some cases, can be found the number of impossible roots.

6. In the *Miscell. Anal.* is given the law of a series, and its demonstration, which finds the sum of the powers of the roots of a given equation from its co-efficients. Mr. EULER has since published the same in the *Peterburg Acts*, Mr. DE LA GRANGE printed a property of this series, also printed by me about the same time; *viz.* that if the series was continued *in infinitum*, the powers would observe the same law as the roots, which indeed immediately follows from the series itself; but from thence with the greatest sagacity he deduces the law of the reversion of the series ($y = a + bx + cx^2 + dx^3 + \&c.$): it has since been given in a different manner from similar principles in the *Medit. Analyt.* 7. In the *Miscell. Analyt.* the law of a series is given for finding the sum of all quantities of this kind $(\alpha^n \times \beta^n \times \gamma^n \times \delta^n \times \&c. + \&c.)$ where $\alpha, \beta, \gamma, \delta, \&c.$ denote the roots of a given equation, from the powers of the roots of the given equation. This law, with a different notation, has been since published in the *Paris Acts* by Mr. VANDERMONDE; who indeed mentions that he

had heard, that a series for that purpose was contained in my book, but had not seen it. In the same book is given a method of finding the aggregates of any algebraical functions of each of the roots of given equations, which is somewhat improved in the latter editions. 8. In the same book are assumed

$$\frac{ax^m + bx^{m-1} + \&c.}{pz^m + qz^{m-1} + \&c.} \text{ and } \frac{Ax^m + Bx^{m-1} + \&c.}{pz^m + qz^{m-1} + \&c.}, \text{ where } x \text{ is any rational}$$

quantity whatever for x and y , the unknown quantities of a given equation of two or more dimensions. 9. In the Miscell. Analyt.

a biquadratic ($x^4 + 2px^2 = qx^2 + rx + s$, of which no term is destroyed) is reduced to a quadratic ($x^2 + px + n =$

$\sqrt{p^2 + 2n + qx} + \sqrt{s + n^2}$;) and in the second edition of it, printed in the years 1767, 1768, 1769, and published in the beginning of the year 1770, the values of n are found

$$\frac{\alpha\beta + \gamma\delta}{2}, \frac{\alpha\gamma + \beta\delta}{2}, \text{ and } \frac{\alpha\delta + \beta\gamma}{2}; \text{ and the six values of } \sqrt{y^2 + 2n + q}$$

respectively $\frac{\alpha + \beta - \gamma - \delta}{2}, \frac{\alpha + \gamma - \beta - \delta}{2}, \frac{\alpha + \delta - \beta - \gamma}{2},$ and their nega-

tives; and the six values of $\sqrt{s + n^2}$ respectively $\frac{\alpha\beta - \gamma\delta}{2}, \frac{\alpha\gamma - \beta\delta}{2},$

$\frac{\alpha\delta - \beta\gamma}{2},$ and their negatives. 10. From a given biquadratic

($y^4 + qy^2 + ry + s = 0$) by assuming $y^2 + ay + b = v$ and a and b such quantities as to make the second and fourth terms of the resulting equations to vanish, there results an equation ($v^2 + Av + B = 0$) of the formula of a quadratic. Mr. DE LA GRANGE has ascribed this resolution to Mr. TSCHIRNHAUSEN; but in the Leipzig Acts the resolution of a cubic is given by Mr. TSCHIRNHAUSEN, but not of a biquadratic: his general design seems to be the extermination of all the terms.

11. Mr.

11. Mr. EULER or Mr. DE LA GRANGE found, that if α be a root of the equation $x^n - 1 = 0$, where n is a prime number, $\alpha, \alpha', \alpha'', \dots, \alpha^{n-1}, 1$ will be (n) roots of it. More on a similar subject has been added in the last edition of the *Medit. Algebr.* 12. It is observed in the *Miscell. Analyt.* that CARDAN's or SCIPIO FERREUS's resolution of a cubic is a resolution of three different cubic equations; and in the *Medit. Algeb.* 1770, the three cubics are given, and the rationale of the resolution (for example: if α, β , and γ , be the roots of the cubic equation $x^3 + qx - r = 0$, then is given the function of the above roots, which are the roots of the reducing equation $x^6 - rx^3 = q^3$); and also the rationale of the common resolution of biquadratics. 13. It is asserted in the *Miscell.* that if the terms $(My^n + by^{n-1}x + cy^{n-2}x^2 + \&c.$ and $Ny^m + By^{m-1}x + Cy^{m-2}x^2 + \&c.)$ of two equations of n and m dimensions, which contain the greatest dimensions of x and y have a common divisor, the equation whose root is x or y , will not ascend to $n \times m$ dimensions; and if the equation, whose root is x or y , ascends to $n \times m$ dimensions, the sum of its roots depends on the terms of n and $n - 1$ dimensions in the one, and m and $m - 1$ dimensions in the other equation, &c. It is also asserted, in the *Miscell.* that if three algebraical equations of n, m , and r dimensions contain three unknown quantities x, y , and z , the equation, whose root is x or y or z , cannot ascend to more than $n \cdot m \cdot r$ dimensions. 14. Mr. BEZOUT has given two very elegant propositions for finding the dimensions of the equation whose root is x or y , &c.; where x, y , &c. are unknown quantities contained in two or more (b) algebraical equations of π, ρ, σ , &c. dimensions, and in which some of the unknown quantities do not ascend to the above π, ρ, σ , &c. dimensions

dimensions respectively. In demonstrating these propositions he uses one (amongst others) before given by me (*viz.* if an equation of n dimensions contains m unknown quantities, the number of different terms which may be contained in it will be $\overline{n+1} \cdot \frac{n+2}{2} \cdot \frac{n+3}{3} \dots \frac{n+m}{m}$). In the *Medit.* 1770 there is given a method of finding in many cases the dimensions of the equation, whose root is x or y , &c.; from which one, if not both, of the above-mentioned cases may more easily be deduced, and others added. 15. In the *Medit.* 1770. is observed, that if there be n equations containing m unknown quantities, where n is greater than m , there will be $n - m$ equations of conditions, &c. 16. In the *Miscell.* is given and demonstrated the subsequent proposition; *viz.* if two equations contain two unknown quantities x and y , in which x and y are similarly involved; the equation, whose root is x or y will have twice the number of roots which the equation, whose root is $x+y$, x^2+y^2 , &c. has. In the *Medit.* 1770 the same reasoning is applied to equations, which have two, three, four, &c. quantities similarly involved. 17. Mr. DE LA GRANGE has done me the honour to demonstrate my method of finding the number of affirmative and negative roots contained in a biquadratic equation. A demonstration of my rule for finding the number of affirmative, negative, and impossible roots contained in the equation $x^4 + Ax^2 + B = 0$ is also omitted, on account of its ease and length. From the *Medit.* the investigation of finding the true number of affirmative and negative roots appears to be as difficult a problem as the finding the true number of impossible roots; and it further appears, that the common methods in both cases can seldom be depended on. But their faults lie on different sides,

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the one generally finds too many, the other too few. 18. In the *Medit.* 1770, from the number of impossible roots in a given equation ($x^n - px^{n-1} + \&c. = 0$) is found the number of impossible roots in an equation, whose roots (v) have any assignable relation to the roots of a given equation; and examples are given in the relation ($nx^{n-1} - \overline{n-1}px^{n-2} + \&c. = v$); and in an equation, whose roots are the squares of the differences of the roots of the given equation. 19. It is observed in the *Medit.* 1770, that in two or more equations, having two or more unknown quantities, the same irrationality will be contained in the correspondent values of each of the unknown quantities, unless two or more values of one of them are equal, &c. The same observation is also applied to the coefficients of an equation deduced from a given equation. 20. In the *Miscell.* was published a new method of exterminating, from a given equation, irrational quantities, by finding the the multipliers, which, multiplied into it, give a rational product. 21. In the *Medit.* 1770, are given the different resolutions of a certain quantity $(a^2 + rb^2)^{2m+1}$ and $(a^2 + rb^2)^{2m+2}$ into quantities of the same kind. 22. Mr. DE LA GRANGE has very elegantly demonstrated Mr. WILSON's celebrated property of prime numbers contained in my book. In the last edition of the *Medit.* the same property is demonstrated, and some similar ones added. 23. In the *Miscell.* is given a method of finding all the integral correspondent values of the unknown quantities of a given simple equation, having two or more unknown quantities; and, in the *Medit.* 1770, are given methods of reducing simple and other algebraical equations into one, so that some unknown quantities may be exterminated; and if the unknown quantities of the resulting equations be integral or rational,

rational, the unknown quantities exterminated may also be integral or rational. 24. In the *Medit.* are given rules for finding the different and correspondent roots of an equation, whose resolution is given. 25. Mr. DE LA GRANGE has recommended my new transformation of equations, published in the *Miscell.* which perhaps is not less general nor elegant than any yet published; and in the *Meditat.* 1770 is given a method very useful in finding the co-efficients.

If either here, or in the preface to the *Medit. Algebraicæ*, I have ascribed to myself any algebraical, or in the properties of curve lines any geometrical, or in the *Medit. Analyt.* any analytical invention, which has been before published by any other person, I can only plead ignorance of it, and shall on the very first conviction acknowledge it.

I must further add, that I have been able to carry my algebraical improvements into geometry; for from them, with some geometrical principles added, I have (unless I am deceived) deduced as many new properties of conic sections and curve lines as have been published by any one since the great geometrician APOLLONIUS.

XXIX. *An Account of a remarkable Frost on the 23d of June, 1783. In a Letter from the Rev. Sir John Cullum, Bart. F. R. S. and S. A. to Sir Joseph Banks, Bart. P. R. S.*

Read May 27, 1784.

DEAR SIR,

Hardwick-house,
Nov. 10, 1783.

WHEN I had the pleasure of seeing you in London, in the autumn, and mentioned a frost that happened in my neighbourhood on the 23d of last June, you expressed a desire of receiving some particulars about it. I therefore now send you some memorandums which I made at the time.

About six o'clock, that morning, I observed the air very much condensed in my chamber-window; and, upon getting up, was informed by a tenant, who lives close to my house, that finding himself cold in bed, about three o'clock in the morning, he looked out at his window, and to his great surprise saw the ground covered with a white frost: and I was afterwards assured, upon indubitable authority, that two men at Barton, about three miles off, saw between three and four o'clock that morning, in some shallow tubs, ice of the thickness of a crown-piece, and which was not melted before six.

This unseasonable frost produced some remarkable effects. The aristæ of the barley, which was coming into ear, became brown and withered at their extremities, as did the leaves of the oats; the rye had the appearance of being mildewed; so that

that the farmers were alarmed for those crops. The wheat was not much affected. The larch, Weymouth pine, and hardy Scotch fir, had the tips of their leaves withered; the first was particularly damaged, and made a shabby appearance the rest of the summer. The leaves of some ashes, very much sheltered in my garden, suffered greatly. A walnut-tree received a second shock (the first was from a severe frost on the 26th of May) which completed the ruin of its crop. Cherry-trees, a standard peach-tree, filbert and hazel-nut trees, shed their leaves plentifully, and littered the walks as in autumn. The barberry-bush was extremely pinched, as well as the hypericum perforatum and hirsutum: as the two last are solstitial, and rather delicate plants, I wondered the less at their sensibility; but was much surprised to find, that the vernal black-thorn and sweet violet, the leaves of which one would have thought must have acquired a perfect firmness and strength, were injured full as much. All these vegetables appeared exactly as if a fire had been lighted near them, that had shrivelled and discoloured their leaves;

— *penetrabile frigus adurit.*

At the time this havock was made among some of our hardy natives, the exotic mulberry-tree was very little affected; a fig-tree, against a north-west wall, remained unhurt, as well as the vine, on the other side, though just coming into blossom. I speak of my own garden, which is high; for in the low ones about Bury, that is but a mile off, the fig-trees, in particular, were very much cut: and, in general, all those gardens suffer more by frost than mine.

Some weather, that was cold for the time of year, had preceded this frost. On the 21st the thermometer had, at no time of the day, risen to 60°; on the 22d, at ten at night, it

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had sunk to 50°. On the last day, and on the 23d, disappeared that dry haze, which had taken place some days before, and continued to blot out the face of the sun for so long a time afterwards. After sun-set on the 24th it appeared again, and the next day the leaves of many vegetables were covered with a clammy sweetness.

The above flight notes were taken in my garden and its environs; and I wish they may afford you the smallest entertainment. If you should think them worth the attention of the Royal Society, dispose of them accordingly. So severe a frost, at so advanced a season, is certainly not one of the least remarkable among the atmospherical phenomena of this year.

I remain, dear Sir,

Your much obliged and faithful servant,

JOHN CULLUM.

XXX. *On a new Method of preparing a Test Liquor to shew the Presence of Acids and Alkalies in chemical Mixtures.* By Mr. James Watt, Engineer; communicated by Sir Joseph Banks, Bart. P. R. S.

Read May 27, 1784.

THE syrop of violets was formerly the test of the point of saturation of mixtures of acids and alkalies, which was principally used; but since the late improvements in chemistry it has been found not to be sufficiently accurate, and the infusion of tournesol, or of an artificial preparation called litmus, have been substituted in the place of it.

The infusion of litmus is blue, and becomes red with acids. It is sensible to the presence of one grain of common oil of vitriol, though it be mixed with 100000 grains of water; but as this infusion does not change its colour on being mixed with alkaline liquors, in order to discover whether a liquor be neutral or alkaline, it is necessary to add some vinegar to the litmus, so as just to turn the infusion red, which will then be restored to its blue colour, by being mixed with any alkaline liquor. The blue infusion of litmus is also a test of the presence of fixed air in water, with which it turns red, as it does with other acids.

The great degree of sensibility of this test would leave very little reason to search for any other, were there reason to believe that it is always a test of the exact point of saturation of

acids and alkalies, which the following fact seems to call in question.

I have observed, that a mixture of phlogisticated nitrous acid with an alkali will appear to be acid, by the test of litmus, when other tests, such as the infusion of the petals of the scarlet rose, of the blue iris, of violets, and of other flowers, will shew the same liquor to be alkaline, by turning green so very evidently as to leave no doubt.

At the time I made this discovery, the scarlet roses and several other flowers, whose petals change their colour by acids and alkalies, were in flower. I stained paper with their juices, and found that it was not affected by the phlogisticated nitrous acid, except in so far as it acted the part of a neutralizing acid; but I found also, that paper, stained in this manner, was by no means so easily affected by acids of any kind as litmus was, and that in a short time it lost much of that degree of sensibility it possessed. Having occasion in winter to repeat some experiments, in which the phlogisticated nitrous acid was concerned, I found my stained paper almost useless. I was, therefore, obliged to search for some substitute among the few vegetables which then existed in a growing state; of these I found the red cabbage (*brassica rubra*) to furnish the best test, and in its fresh state to have more sensibility both to acids and alkalies than litmus; and to afford a more decisive test, from its being naturally blue, turning green with alkalies, and red with acids; to which is joined the advantage of its not being affected by phlogisticated nitrous acid any farther than it acts as a real acid.

To extract the colouring matter, take those leaves of the cabbage, which are freshest, and have most colour; cut out the larger stems, and mince the thin parts of the leaves very small; then digest them in water, about the heat of 120 degrees, for

a few hours, and they will yield a blue liquor, which, if used immediately as a test, will be found to possess great sensibility. But, as this liquor is very subject to turn acid and putrid, and to lose its sensibility, when it is wanted to be preserved for future use the following processes succeed the best.

1. After having minced the leaves, spread them on paper, and dry them in a gentle heat; when perfectly dry, put them up in glass bottles well corked; and when you want to use them, acidulate some water with vitriolic acid, and digest, or infuse, the dry leaves in it until they give out their colour; then strain the liquor through a cloth, and add to it a quantity of fine whiting or chalk, stirring it frequently until it becomes of a true blue colour, neither inclining to green nor purple; as soon as you perceive that it has acquired this colour, filter it immediately, otherwise it will become greenish by longer standing on the whiting.

This liquor will deposite a small quantity of gypsum, and by the addition of a little spirit of wine will keep good for some days, after which it will become a little putrid and reddish. If too much spirit is added, it destroys the colour. If the liquor is wanted to be kept longer, it may be neutralized by means of a fixed alkali instead of chalk.

2. But as none of these means will preserve the liquor long without requiring to be neutralized afresh, just before it is used; and as the putrid and acid fermentation which it undergoes, and perhaps the alkalies or spirit of wine mixed with it, seem to lessen its sensibility; in order to preserve its virtues while it is kept in a liquid state, some fresh leaves of the cabbage, minced as has been directed, may be infused in a mixture of vitriolic acid and water, of about the degree of acidity of vinegar; and it may be neutralized, as it is wanted, either by means of chalk,

or

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or of the fixed or volatile alkali. But it is necessary to observe, that if the liquor has an excess of alkali, it will soon lose its colour, and become yellow, from which state it cannot be restored; therefore care should be taken to bring it very exactly to a blue, and not to let it verge towards a green *.

3. By the same process I have made a red infusion of violets, which, on being neutralized, forms at present a very sensible test; but how long it will preserve its properties I have not yet determined. Probably the coloured infusions of other flowers may be preserved in the same manner, by the antiseptic power of the vitriolic acid, so as to lose little of their original sensibility. Paper, fresh stained with these tests in their neutral state, has sufficient sensibility for many experiments; but the alum and glue which enter into the preparation of writing-paper seem in some degree to fix the colour; and paper which is not sized becomes somewhat transparent, when wetted, which renders small changes of colour imperceptible; so that where accuracy is required, the test should be used in a liquid state †.

* Since writing the above, I have found, that the infusions of red cabbages and of various flowers in water acidulated by means of vitriolic acid, are apt to turn mouldy in the summer season, and also that the moulding is prevented by the addition of spirits of wine. The quantity of spirit which is necessary for this purpose I have not been able to ascertain; but I add it by little at a time, until the progress of the moulding is prevented.

† I have found, that the petals of the scarlet rose, and those of the pink-coloured lychnis, treated in this manner, afford very sensible tests.



XXXI. *An Account of a new Plant, of the Order of Fungi.*
By Thomas Woodward, Esq; communicated by Sir Joseph
Banks, Bart. P. R. S.

Read June 10, 1784.

Plantæ novæ Descriptio — an Genus novum?

Radiçes paucæ; tenues; albidæ.

Volva ovata; duplex, mucilagine interposita; subalbida.

Stipes, e volva interiore surgens, sublignosus; cavus;
cortice lacerato vestitus; subfuscus.

Capitulum, stipitis summitati insidens, reflexum; subtus
campanulatum, glabrum; superne pulverulentum; et, e
pulveris crassitie, globiforme; volvæ ruptæ summita-
tem, minime adhærentem, in se gerens.

Pulvis sphaericus; semipellucidus; luteo-fuscus.

THIS extraordinary vegetable production arises from a volva, which is buried six or eight inches deep in dry sandy banks; and, consequently, it is extremely difficult to detect it in its earliest state. At its first appearance above ground, the powdery head is covered with a loose campanulated cap, which does not adhere by any the smallest filaments; and which, I suppose, to be the upper part of the volva, as both always appear ragged when taken up. When the plant is taken up immediately on its appearing above ground, the stem is about six or eight inches long; and, as well as the volva, replete with

with mucilage, making it much heavier than when it has attained its full growth. This is the state to which the description given above refers. The dust is now perfectly formed, and is dispersed by the slightest touch, or by the wind. A great alteration soon takes place, as it now proceeds very rapidly, and in a few days attains the summit of its growth, which is from nine to fifteen inches, more than half being generally buried in the ground. The stem becomes woody, though hollow, the bark still more ragged, and the whole plant much lighter, both volva and stem being now quite dry, and free from mucilage. The wind and showers soon disperse the greatest part of the dust; and at length the stalk appears with a naked, coriaceous, campanulated pileus, and considerably bleached, in colour and appearance not unlike a dry stalk of hemp. In this state some of them are now to be found (Aug. 28, 1783) with plants of this year rising near them.

Mr. HUMPHREYS, of Norwich, who first found this very extraordinary plant, met with it only in the state last described, and without discovering the volva; so that no judgment of it could be formed. It has been taken by some persons for a decayed or abortive agaric; but that opinion could not be maintained by any one who had seen it in its recent state,

I first met with it in February or March 1783 in its dry and withered state; but as it was suspected, though with little appearance of reason, to be a decayed *Agaricus procerus*, I wished to examine the root carefully, in order to observe whether it was bulbous. The bulb of the *Agaricus procerus* is scarcely hidden under the surface, and I was much surprised at the depth to which I was obliged to search for the root of
this

this plant; at length, however, removing the earth carefully to the depth of seven or eight inches, I met with it, and to my great pleasure and surprise, on raising the plant, I discovered the volva, which was so unlike the fugitive one of the agaric, that I was immediately convinced it must be something new.

An account of this was directly sent to Mr. DICKSON, of Covent-Garden, an able botanist, and diligent enquirer after the class Cryptogamia. Mr. DICKSON, who had before seen it in the state in which it was found by Mr. HUMPHREYS, but could make nothing of it, though thoroughly convinced it was no agaric, immediately requested that I would watch the spot, and endeavour to detect the plant in its earliest appearance. I communicated this to my neighbour Mr. STONE, a most diligent and skilful botanist, who first restored the *Lycoperdon coliforme*; and we determined to examine the spot carefully together, from the month of August downward. About the middle of August we first discovered a plant just arisen, which was sent to Mr. DICKSON, and a full description of which is before given; but though we have daily visited the spot since, we have never been able to find it again in so young a state; for so rapid appears to be its growth, that we have found plants of two or three inches height above the ground, the stems of which had lost part of their mucilage, where the day before none had been visible. We have three or four times attempted to discover the volva in its earliest state, by removing the earth carefully near the old stems of the preceding year; but this has been without success: and there is little hope of succeeding in it, as the volva lies very deep in the ground, and the plant arises at such various times.

This plant agrees with the genus *Phallus* in its volva, which has a double coat replete with mucilage; and its stipes crowned with a reflexed pileus. But it more nearly approaches the genus *Lycoperdon*, by its head covered with a thick dust, contained in a substance of a spongy appearance, and by the form of the dust, which agrees perfectly with that of most of the true lycoperdons, when examined in the microscope. To this genus it must at present probably be referred, though the total want of an exterior coat prevents its agreeing with it so perfectly as it ought.

The *Mucor* * *septicus* of HUDSON and LIGHTFOOT (*Mucor ovatus* of SCHÆFFER); the *Mucor* * *butyraceus* of SCHÆFFER (194.), not taken notice of by either HUDSON or LIGHTFOOT, but which I have often found here; and the *Lycoperdon* * *epidendrum* of LIGHTFOOT, which I suppose to be what HUDSON calls *Lycoperdon epiphyllum*, as he has referred to the same plate of SCHÆFFER (193. *Mucor fragiformis*); have all some affinity with the fructification of this plant; and the more so, if we suppose the head to be at first covered with a mucilage, which afterwards turns to a dust; but this will hardly be admitted, as the plant sent to Mr. DICKSON had the dust perfectly formed, though the volva and stem were both replete with mucilage. But we cannot admit it to agree with any of these last mentioned plants, as they have all an exterior coat, though very fugitive, of which this seems entirely destitute. We may add, that they are all very fugitive productions;

* I cannot help observing that, in my opinion, HALLER has done more rightly in making these into a new genus (*Filago*), than our botanists, who have jumbled them with the genera *Lycoperdon* and *Mucor*, to which they have no great affinity any more than the *Sphæria* of HALLER, likewise very improperly ranked with the *Lycoperdons* and *Clavarias*.

whereas this, though soon arriving at maturity, is of a woody and permanent structure.

P. S. In a letter Mr. DICKSON received from Mr. WOODWARD, Feb. 12, 1784, he informs him, that he is quite convinced by some late observations, that the above-mentioned plant frequently comes to a state of perfection before it reaches the surface. The only difference to be observed is, that the dust in that case is of a darker colour, which he supposes is owing to its not being exposed to the air.

XXXII. *Experiments to investigate the Variation of Local Heat.*

By James Six, Esq.; communicated by the Rev. Francis Wollaston, LL.B. F. R. S.

Read June 10, 1784.

BEING desirous of investigating the variation of local heat, I made the following experiments.

On the 4th of September, 1783, I placed thermometers in three different stations; one on the top of the high tower of Canterbury Cathedral, about 220 feet from the ground; another at the bottom of the same tower, at about 110 feet; and a third in my own garden *, not more than six feet from the ground. They were all carefully exposed to the open air in a shady northern aspect; the lowest was as little liable to be affected by the reflection of the sun's rays as the elevation would permit, the second still less, and the highest not at all. They continued unremoved in their several places, where I visited them daily for the space of three weeks, and minuted down the greatest degree of heat and cold that happened each day and night in their respective stations †.

* This garden is situate not far from the Cathedral, at the extremity of the buildings on the north side of the city.

† The thermometers here made use of were constructed to shew the greatest degree of heat and cold which happened in the observer's absence (described Phil. Transf. vol. LXXII. part I.), which rendered them particularly convenient on this occasion. They had hung together for some time, and seldom differed half a degree from each other.

By

By these observations it appears, see Table I. that, notwithstanding some irregularities, the heat of the days at the lowest station always exceeded that at the middle, and still more the heat at the upper station. As in many instances the higher regions of the atmosphere have been found to be colder than the lower, and the thermometer in the garden was more liable to be heated by the reflection of the sun's rays from the earth than the upper ones, a difference of this kind might have been expected. But I was greatly surprised to find the cold of the night at the lowest, not only equal to, but, very frequently, exceeding the cold at the higher stations. As I wished to know, whether these variations would continue the same in the winter, when the weather was colder; and whether a thermometer, placed at some distance from the city, having an elevation equal to that on the top of the Cathedral tower, would agree with it; on the 19th of December, 1783, I disposed the three thermometers in the following manner: one in my garden; one on the top of the high tower, as before; and the third on the top of St. Thomas's Hill, about a mile distant from the city, where, at fifteen feet from the ground, it was nearly level with that on the Cathedral tower. Table II. contains the observations that were then made*. The weather at this time proving cold, favoured the experiment; and I now found the several thermometers nearly agreeing with each other in the day-time; but in the night, the cold at the lower station exceeded the cold at the higher ones rather more than it did in the month of September, when the weather was warmer.

* The few omissions in this Table were occasioned by the severity of the cold preventing my attending at a proper time the thermometers, which were at a considerable distance from each other.

At

At the time of taking these thermometrical observations, I likewise noted the different dispositions of the atmosphere in other respects: such as the pressure, moisture, and dryness of the air; force and direction of the winds; quantity of rain; whether the appearances of the sky were clear or cloudy, &c. as I apprehended the local variation of the thermometers might, in a certain degree, correspond with some particular change in the state of the atmosphere.

The event answered my expectation in a singular manner in respect to the nocturnal variation; for it generally happened, that when the sky was dark and cloudy, whatever was the condition of the atmosphere with relation to the other particulars above enumerated, the thermometers agreed pretty nearly with each other; but, on the contrary, whenever the sky became clear, the cold of the night at the lowest station in the garden, constantly exceeded the cold at the top of the Cathedral tower, where the instrument was placed 220 feet from the ground, entirely exposed to the open air, wind, dews, and rain, in a shady northern aspect.

The local variations in the day-time seemed to be regulated by the general degree of heat only, without being affected by any other particular disposition of the atmosphere, or the clearness or cloudiness of the sky, as the nocturnal variations were. In the month of September, when the glasses rose from 60° to 70° , the heat at the lower station constantly exceeded the heat at the upper station; and in some measure proportionally, as the weather was hotter*. In December and January, when

* As the heat at the lower station exceeded the heat at the upper ones, when the weather was hot; and equally so, whenever the sky was cloudy, as well as when it was clear; it appears, that the glass at the lower station was not materially affected by the reflection of the sun's rays from the earth, as at first I apprehended it would be.

from

from below 30° they seldom rose to 40° , the local variation in the day-time nearly ceased, or was found in very small degrees inclining sometimes one way, sometimes the other.

That the clearness of the sky should contribute to the coolness of the air in the night, is not at all surprising; but that, whenever the sky becomes clear, the cold should seem to arise from the earth, and be found in the greatest degree, as long as it continues clear, in the lowest situation, seems a little extraordinary: this, however, appeared to be the case, both in the warmer as well as in the colder weather, during the whole time these observations were taken, and remarkably so on the following days. On the first of January the weather was cold, the sky cloudy, the glasses in the night were at 20° , and in the day at 34° : the wind which had been at S.E. the day before, changed in the evening to S. and brought on a thaw. On the second of January clouds and misty rain darkened the sky all day; the wind blew briskly at S.W.; the glasses in the night were at $32^{\circ}\frac{1}{2}$, in the day at 40° . On the third of January the clouds and rain continued, the weather growing still warmer; wind at S.W. by S.; the glasses in the night were at 36° , in the day at $45^{\circ}\frac{1}{2}$. These three days the weather gradually became warmer; and, while the sky remained darkened by clouds, all the glasses in their several stations nearly agreed with each other. About noon, on the third of January, the sky becoming clear, the air grew cooler; and going into my garden, about eight o'clock in the evening, I perceived the surface of the ground, which had been wet by the rain in the forenoon, began to be frozen. Looking immediately at the thermometer, I saw the mercury at $33^{\circ}\frac{1}{2}$; and observing a piece of wet linen hanging near the glass, not five feet from the ground, I took it into my hand, and found it not in the least frozen; by which it appeared,
that

that the degree of cold which had frozen the surface of the ground, had not then ascended to the glass, nor to the linen, and consequently had not been communicated to the air five or six feet above the earth. The next day I found, as I expected, a considerable local variation; the index for the cold of the night in the garden being at 32° , that on the hill being at $35^{\circ}\frac{1}{2}$, and that on the top of the tower at $37^{\circ}\frac{1}{2}$ *. Probably the weather did not continue clear the whole night; if it had, it is likely the degrees of cold would have been found proportionally greater at every station. On the morning of the 4th there fell a misty rain, which continued only till noon, when the sky became clear again, and continued so till the 7th; during which time the nocturnal heights of the thermometers differed considerably from each other; but on the sky's becoming cloudy, the local variation ceased.

Thermometrical observations, made under the same circumstances in respect to the season of the year, place, and situation†, may probably be liable to similar local varia-

* It is remarkable, that the thermometer on St. Thomas's hill did not vary so much from that in the garden, as that did which was on the Cathedral tower, although these two elevated glasses were within three feet of a perfect level with each other; the variations, however, as often as they happened, inclined the same way. The reason of this might probably be, that although the glass on the hill was at an equal altitude with that on the tower, in respect to the ground on which the Cathedral stands; yet the former was only 15 feet, while the latter was 220 feet from the ground.

† Situation in regard to hill or valley. The valley in which Canterbury stands is at that place about a mile in breadth, opening to the N.E.; the hills on either side do not rise very sudden, nor very high; the river Stour, divided into branches, passes through the city, and, about fourteen miles below, empties itself into the sea, which washes the coast from the NN.W. round by the E. to the S.; distant from the city at different places from six to sixteen miles.

tions : to those who make them, the result of these experiments may be of some use. If convenient opportunity offered, I should be glad, by the assistance of friends, to try the local difference of heat and cold in more distant, as well as more elevated, situations.

By experiments of this kind it may possibly in some measure be found, how far evaporations from the earth, at certain times, or vapours ascending, descending, or meeting, in different parts of the atmosphere, may increase or diminish the heat of the air in those places : or whether different degrees of heat and cold (subject however to change) may not be found in different strata of air, or vapour, floating in different parts of the atmosphere ; or in what degree and proportion, the cold increases at different altitudes and in different seasons of the year : whether the cold, which is known to be very intense in the summer time on the tops of high mountains, receives a proportional increase, or be not less subject to variety by the return of winter and summer, night and day, than what we experience in the plains below.

March 10, 1784.

JAMES SIX.

Mr. SIX's Experiments to investigate the Variation

T A B L E I.

The greatest daily variation of heat and cold in the atmosphere, from 1783, taken from three different stations, and compared together: the tower in Canterbury, 220 feet from the ground; another at the bottom of the tower; and a third in a garden, about six feet from the ground. N. B. The night immediately preceding the day to the date of which they are

	Greatest degree of cold in the night.						Greatest degree of heat in the day.				
	Thermometer in the garden.	Thermometer at the bottom of the tower.	Thermometer on the top of the tower.	Difference of garden from bottom of tower.	Difference of garden from top of tower.		Thermometer in the garden.	Thermometer at the bottom of the tower.	Thermometer on the top of the tower.	Difference of garden from bottom of tower.	Difference of garden from top of tower.
Sept.											
4	50½	—	51	—	-0½		66	61	61	+5	+5
5	48	47½	47½	+0½	+0½		62½	61½	61½	+1	+1
6	48½	50	50½	-1½	-2		66½	65½	64½	+1½	+2
7	48	49½	49½	-1½	-1½		63½	62½	63	+1	+0½
8	50	51	51	-1	-1		66	62½	62	+3½	+4
9	53½	55½	55½	—	—		65	63	62½	+2	+2½
10	45	47	47½	2	-2½		63½	59½	59½	+4	+4
11	42	45	45½	-3	-3½		63½	62	60½	+1½	+3
12	52½	53½	54	-1	-1½		69	66½	65	+2½	+4
13	45	48	48½	-3	-3½		65	62	62	+3	+3

in the 4th to the 24th of September,
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1783, to the
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 N. B. The

Morning still and foggy; wind began to blow in the forenoon at S.W.; clouds and rain in the afternoon and night; bar. 29.3.	
Morning cloudy; heavy rain; clear in the afternoon; wind high at W.N.W.; bar. 29.3.	
Morning rained a little; wind very high at S.W. most part of the day; bar. 29.5.	
Morning clear; continued so most part of the day; wind very high at W.; bar. 29.8.	
Sometimes clear, sometimes cloudy; wind very high at W.; bar. 29.9.	
Morning close and cloudy; clear at noon; wind brisk at S.W.; bar. 29.5.	
Morning and great part of the day clear; wind high at S.W.; evening clear; bar. 29.6.	
Morning clear; cloudy about noon; brisk wind at S.; evening still; bar. 29.8.	
Morning cloudy; wind high at S.; evening and clear; bar. 29.4.	
clear, a little rain at noon; cloudy ; wind brisk at S.; bar. 29.0.	

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57	57½	14

XXXIII. *Account of some Observations tending to investigate the Construction of the Heavens.* By William Herschel, Esq.
F. R. S.

Read June 17, 1784.

IN a former paper I mentioned, that a more powerful instrument was preparing for continuing my reviews of the heavens. The telescope I have lately completed, though far inferior in size to the one I had undertaken to construct when that paper was written, is of the Newtonian form, the object speculum being of 20 feet focal length, and its aperture 18 $\frac{1}{2}$ inches. The apparatus on which it is mounted is contrived so as at present to confine the instrument to a meridional situation, and by its motions to give the right-ascension and declination of a celestial object in a coarse way; which, however, is sufficiently accurate to point out the place of the object, so that it may be found again. It will not be necessary to enter into a more particular description of the apparatus, since the account I have now the honour of communicating to the Royal Society regards rather the performance of the telescope than its construction.

It would, perhaps, have been more eligible to have waited longer, in order to complete the discoveries that seem to lie within the reach of this instrument, and are already, in some respects, pointed out to me by it. By taking more time I could undoubtedly be enabled to speak more confidently of the

interior construction of the heavens, and its various *nebulous and sidereal strata* (to borrow a term from the natural historian) of which this paper can as yet only give a few outlines, or rather hints. As an apology, however, for this prematurity, it may be said, that the end of all discoveries being communication, we can never be too ready in giving facts and observations, whatever we may be in reasoning upon them.

Hitherto the sidereal heavens have, not inadequately for the purpose designed, been represented by the concave surface of a sphere, in the center of which the eye of an observer might be supposed to be placed. It is true, the various magnitudes of the fixed stars even then plainly suggested to us, and would have better suited the idea of an expanded firmament of three dimensions; but the observations upon which I am now going to enter still farther illustrate and enforce the necessity of considering the heavens in this point of view. In future, therefore, we shall look upon those regions into which we may now penetrate by means of such large telescopes, as a naturalist regards a rich extent of ground or chain of mountains, containing strata variously inclined and directed, as well as consisting of very different materials. A surface of a globe or map, therefore, will but ill delineate the interior parts of the heavens.

It may well be expected, that the great advantage of a large aperture would be most sensibly perceived with all those objects that require much light, such as the very small and immensely distant fixed stars, the very faint nebulae, the close and compressed clusters of stars, and the remote planets.

On applying the telescope to a part of the *via lactea*, I found that it completely resolved the whole whitish appearance into small stars, which my former telescopes had not light enough

to effect. The portion of this extensive tract, which it has hitherto been convenient for me to observe, is that immediately about the hand and club of Orion. The glorious multitude of stars of all possible sizes that presented themselves here to my view was truly astonishing; but, as the dazzling brightness of glittering stars may easily mislead us so far as to estimate their number greater than it really is, I endeavoured to ascertain this point by counting many fields, and computing, from a mean of them, what a certain given portion of the milky way might contain. Among many trials of this sort I found, last January the 18th, that six fields, promiscuously taken, contained 110, 60, 70, 90, 70, and 74 stars each. I then tried to pick out the most vacant place that was to be found in that neighbourhood, and counted 63 stars. A mean of the first six gives 79 stars for each field. Hence, by allowing 15 minutes of a great circle for the diameter of my field of view, we gather, that a belt of 15 degrees long and two broad, or the quantity which I have often seen pass through the field of my telescope in one hour's time, could not well contain less than fifty thousand stars, that were large enough to be distinctly numbered. But, besides these, I suspected at least twice as many more, which, for want of light, I could only see now and then by faint glittering and interrupted glimpses,

The excellent collection of nebulae and clusters of stars which has lately been given in the *Connoissance des Temps* for 1783 and 1784, leads me next to a subject which, indeed, must open a new view of the heavens. As soon as the first of these volumes came to my hands, I applied my former 20-foot reflector of 12 inches aperture to them; and saw, with the greatest pleasure, that most of the nebulae, which I had an opportunity of examining in proper situations, yielded to the

force of my light and power, and were resolved into stars. For instance, the 2d, 5, 9, 10, 12, 13, 14, 15, 16, 19, 22, 24, 28, 30, 31, 37, 51, 52, 53, 55, 56, 62, 65, 66, 67, 71, 72, 74, 92, all which are said to be *nebulæ* without stars, have either plainly appeared to be nothing but stars, or at least to contain stars, and to shew every other indication of consisting of them entirely. I have examined them with a careful scrutiny of various powers and light, and generally in the meridian. I should mention; that five of the above, *viz.* the 16th, 24, 37, 52, 67, are called clusters of stars containing *nebulosity*; but my instrument resolving also that portion of them which is called *nebulous* into stars of a much smaller size, I have placed them into the above number. To these may be added the 1st, 3d, 27, 33, 57, 79, 81, 82, 101, which in my 7, 10, and 20-feet reflectors shewed a mottled kind of *nebulosity*, which I shall call *resolvable*; so that I expect my present telescope will, perhaps, render the stars visible of which I suppose them to be composed. Here I might point out many precautions necessary to be taken with the very best instruments, in order to succeed in the resolution of the most difficult of them; but reserving this at present too extensive subject for a future opportunity, I proceed to speak of the effects of my last instrument with regard to *nebulæ*.

My present pursuits, as I observed before, requiring this telescope to act as a fixed instrument, I found it not convenient to apply it to any other of the *nebulæ* in the *Connoissance des Temps* but such as came in turn; nor, indeed, was it necessary to take any particular pains to look for them, it being utterly impossible that any one of them should escape my observation when it passed the field of view of my telescope. The few which I have already had an opportunity of examining, shew plainly that

those

those most excellent French astronomers, Mess. MESSIER and MECHAIN, saw only the more luminous part of their nebulae; the feeble shape of the remainder, for want of light, escaping their notice. The difference will appear when we compare my observation of the 98th nebula with that in the *Connoissance des Temps* for 1784, which runs thus: “Nébuleuse sans étoile, d’une lumière extrêmement foible, au dessus de l’aile boréale de la Vierge, sur le parallèle et près de l’étoile N° 6. cinquième grandeur, de la chevelure de Bérénice, suivant FLAMSTEED. M. MECHAIN la vit le 15 Mars, 1781.” My observation of the 30th of December, 1783, is thus: A large, extended, fine nebula. Its situation shews it to be M. MESSIER’s 98th; but from the description it appears, that that gentleman has not seen the whole of it, for its feeble branches extend above a quarter of a degree, of which no notice is taken. Near the middle of it are a few stars visible, and more suspected. My field of view will not quite take in the whole nebula. See fig. 1. tab. XVII. Again, N° 53. “Nébuleuse sans étoiles, découverte au-dessous et près de la chevelure de Bérénice, à peu de distance de l’étoile quarante-deuxième de cette constellation, suivant FLAMSTEED. Cette nébuleuse est ronde et apparente, &c.” My observation of the 170th Sweep runs thus: A cluster of very close stars; one of the most beautiful objects I remember to have seen in the heavens. The cluster appears under the form of a solid ball, consisting of small stars, quite compressed into one blaze of light, with a great number of loose ones surrounding it, and distinctly visible in the general mass. See fig. 2.

When I began my present series of observations, I surmised, that several nebulae might yet remain undiscovered, for want of sufficient light to detect them; and was, therefore, in hopes
of

of making a valuable addition to the clusters of stars and nebulae already collected and given us in the work before referred to, which amount to 103. The event has plainly proved that my expectations were well founded; for I have already found 466 new nebulae and clusters of stars, none of which, to my present knowledge, have been seen before by any person; most of them, indeed, are not within the reach of the best common telescopes now in use. In all probability many more are still in reserve; and as I am pursuing this track, I shall make them up into separate catalogues, of about two or three hundred at a time, and have the honour of presenting them in that form to the Royal Society.

A very remarkable circumstance attending the nebulae and clusters of stars is, that they are arranged into strata, which seem to run on to a great length; and some of them I have already been able to pursue, so as to guess pretty well at their form and direction. It is probable enough, that they may surround the whole apparent sphere of the heavens, not unlike the milky way, which undoubtedly is nothing but a stratum of fixed stars. And as this latter immense starry bed is not of equal breadth or lustre in every part, nor runs on in one straight direction, but is curved and even divided into two streams along a very considerable portion of it; we may likewise expect the greatest variety in the strata of the clusters of stars and nebulae. One of these nebulous beds is so rich, that, in passing through a section of it, in the time of only 36 minutes, I detected no less than 31 nebulae, all distinctly visible upon a fine blue sky. Their situation and shape, as well as condition, seems to denote the greatest variety imaginable. In another stratum, or perhaps a different branch of the former, I have seen double and treble nebulae, variously arranged; large ones with

with small, seeming attendants; narrow but much extended, lucid nebulae or bright dashes; some of the shape of a fan, resembling an electric brush, issuing from a lucid point; others of the cometic shape, with a seeming nucleus in the center; or like cloudy stars, surrounded with a nebulous atmosphere; a different sort again contain a nebulosity of the milky kind, like that wonderful, inexplicable phenomenon about θ Orionis; while others shine with a fainter, mottled kind of light, which denotes their being resolvable into stars. See fig. 3. &c. But it would be too extensive at present to enter more minutely into such circumstances, therefore I proceed with the subject of nebulous and sidereal strata.

It is very probable, that the great stratum, called the milky way, is that in which the sun is placed, though perhaps not in the very center of its thickness. We gather this from the appearance of the Galaxy, which seems to encompass the whole heavens, as it certainly must do if the sun is within the same. For, suppose a number of stars arranged between two parallel planes, indefinitely extended every way, but at a given considerable distance from each other; and, calling this a sidereal stratum, an eye placed somewhere within it will see all the stars in the direction of the planes of the stratum projected into a great circle, which will appear lucid on account of the accumulation of the stars; while the rest of the heavens, at the sides, will only seem to be scattered over with constellations, more or less crowded, according to the distance of the planes or number of stars contained in the thickness or sides of the stratum.

Thus, in fig. 16. (tab. XVIII.) an eye at S within the stratum ab , will see the stars in the direction of its length ab , or height cd , with all those in the intermediate situations, projected into the

lucid circle ACBD; while those in the sides *mv*, *nw*, will be seen scattered over the remaining part of the heavens at MVNW.

If the eye were placed somewhere without the stratum, at no very great distance, the appearance of the stars within it would assume the form of one of the less circles of the sphere, which would be more or less contracted to the distance of the eye; and if this distance were exceedingly increased, the whole stratum might at last be drawn together into a lucid spot of any shape, according to the position, length, and height of the stratum.

Let us now suppose, that a branch, or smaller stratum, should run out from the former, in a certain direction, and let it also be contained between two parallel planes extended indefinitely onwards, but so that the eye may be placed in the great stratum somewhere before the separation, and not far from the place where the strata are still united. Then will this second stratum not be projected into a bright circle like the former, but will be seen as a lucid branch proceeding from the first, and returning to it again at a certain distance less than a semi-circle.

Thus, in the same figure, the stars in the small stratum *pq* will be projected into a bright arch at PRRP, which, after its separation from the circle CBD, unites with it again at P.

What has been instanced in parallel planes may easily be applied to strata irregularly bounded, and running in various directions; for their projections will of consequence vary according to the quantities of the variations in the strata and the distance of the eye from the same. And thus any kind of curvatures, as well as various different degrees of brightness, may be produced in the projections.

From appearances then, as I observed before, we may infer, that the sun is most likely placed in one of the great strata of the fixed stars, and very probably not far from the place where some smaller stratum branches out from it. Such a supposition will satisfactorily, and with great simplicity, account for all the phenomena of the milky way, which, according to this hypothesis, is no other than the appearance of the projection of the stars contained in this stratum and its secondary branch. As a farther inducement to look on the Galaxy in this point of view, let it be considered, that we can no longer doubt of its whitish appearance arising from the mixed lustre of the numberless stars that compose it. Now, should we imagine it to be an irregular ring of stars, in the center nearly of which we must then suppose the sun to be placed, it will appear not a little extraordinary, that the sun, being a fixed star like those which compose this imagined ring, should just be in the center of such a multitude of celestial bodies, without any apparent reason for this singular distinction; whereas, on our supposition, every star in this stratum, not very near the termination of its length or height, will be so placed as also to have its own Galaxy, with only such variations in the form and lustre of it, as may arise from the particular situation of each star.

Various methods may be pursued to come to a full knowledge of the sun's place in the sidereal stratum, of which I shall only mention one as the most general and most proper for determining this important point, and which I have already begun to put in practice. I call it *Gaging the Heavens*, or the *Star-Gage*. It consists in repeatedly taking the number of stars in ten fields of view of my reflector very near each other, and by adding their sums, and cutting off one decimal on the right, a mean of the contents of the heavens, in all the parts which

M m m 2

are

re thus gaged, is obtained. By way of example, I have joined a short table, extracted from the gages contained in my journal, by which it appears, that the number of stars increases very fast as we approach the Via Lactea.

N. P. D. 92 to 94			N. P. D. 78 to 80		
R. A.		Gage.	R. A.		Gage.
15	10	9.4	11	16	3.1
15	22	10.6	12	31	3.4
15	47	10.6	12	44	4.6
16	8	12.1	12	49	3.9
16	25	13.6	13	5	3.8
16	37	18.6	14	30	3.6

Thus, in the parallel from 92 to 94 degrees north polar distance, and R. A. 15 h. 10', the star-gage runs up from 9.4 stars in the field to 18.6 in about an hour and a half; whereas in the parallel from 78° to 80° north polar distance, and R. A. 11, 12, 13, and 14 hours, it very seldom rises above 4. We are, however, to remember, that with different instruments the account of the gages will be very different, especially on our supposition of the situation of the sun in a stratum of stars. For, let ab , fig. 17. be the stratum, and suppose the small circle $g b / k$ to represent the space into which, by the light and power of a given telescope, we may penetrate; and let $GHLK$ be the extent of another portion, which we are enabled to visit by means of a larger aperture and power; it is evident, that the gages with the latter instrument will differ very much in their account of stars contained at MN , and at KG or LH ; when with the former they will hardly be affected by the change from mn to kg or lb . And this accounts for what a celebrated author says concerning the effects of telescopes,

scopes, by which we must understand the best of those that are in common use *.

It would not be safe to enter into an application of these, and such other gages as I have already taken, till they are sufficiently continued and carried all over the heavens. I shall, therefore, content myself with just mentioning that the situation of the sun will be obtained, from considering in what manner the star-gage agrees with the length of a ray revolving in several directions about an assumed point, and cut off by the bounds of the stratum. Thus, in fig. 18. let S be the place of an observer; $Srrr$, $Srrr$, lines in the planes rSr , rSr , drawn from S within the stratum to one of the boundaries, here represented by the plane AB. Then, since neither the situation of S, nor the form of the limiting surface AB, is given, we are to assume a point, and apply to it lines proportional to the several gages that have been obtained, and at such angles from each other as they may point out; then will the termination of these lines delineate the boundary of the stratum, and consequently manifest the situation of the sun within the same. But to proceed.

If the sun should be placed in the great sidereal stratum of the milky way, and, as we have surmised above, not far from

* On voit avec les télescopes des étoiles dans toutes les parties du ciel, à peu près comme dans la voie lactée, ou dans les nébuleuses. On ne sauroit douter qu'une partie de l'éclat et de la blancheur de la voie lactée, ne provienne de la lumière des petites étoiles qui s'y trouvent en effet par millions; cependant, avec les plus grands télescopes, on n'en distingue pas assez, et elles n'y sont pas assez rapprochées les unes des autres pour qu'on puisse attribuer à celles qu'on distingue la blancheur de la voie lactée, si sensible à la vue simple. L'on ne sauroit donc prononcer que les étoiles soient la seule cause de cette blancheur, quoique nous ne connoissions aucune manière satisfaisante de l'expliquer. *Ast. M. DE LA LANDE*, § 883.

the

the branching out of a secondary stratum, it will very naturally lead us to guess at the cause of the probable motion of the solar system: for the very bright, great node of the Via Lactis, or union of the two strata about Cepheus and Cassiopeia, and the Scorpion and Sagittarius, points out a conflux of stars manifestly quite sufficient to occasion a tendency towards that node in any star situated at no very great distance; and the secondary branch of the Galaxy not being much less than a semi-circle seems to indicate such a situation of our solar system in the great undivided stratum as the most probable.

What has been said in a former paper on the subject of the solar motion seems also to support this supposed situation of the sun; for the apex there assigned lies nearly in the direction of a motion of the sun towards the node of the strata. Besides, the joining stratum making a pretty large angle at the junction with the primary one, it may easily be admitted, that the motion of a star in the great stratum, especially if situated considerably towards the side farthest from the small stratum, will be turned sufficiently out of the straight direction of the great stratum towards the secondary one. But I find myself insensibly led to say more on this subject than I am as yet authorised to do; I will, therefore, return to those observations which have suggested the idea of celestial strata.

In my late observations on nebulae I soon found, that I generally detected them in certain directions rather than in others; that the spaces preceding them were generally quite deprived of their stars, so as often to afford many fields without a single star in it; that the nebulae generally appeared some time after among stars of a certain considerable size, and but seldom among very small stars; that when I came to one nebula, I generally found several more in the neighbourhood; that afterwards

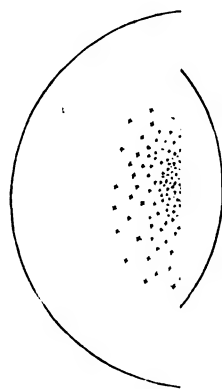
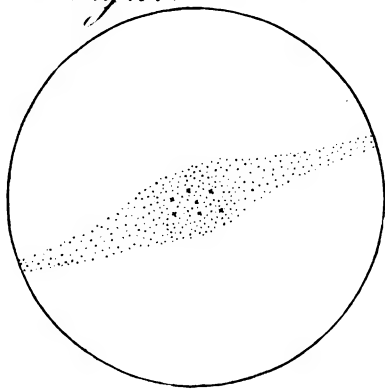
wards a considerable time passed before I came to another parcel; and these events being often repeated in different altitudes of my instrument, and some of them at a considerable distance from each other, it occurred to me, that the intermediate spaces between the sweeps might also contain *nebulæ*; and finding this to hold good more than once, I ventured to give notice to my assistant at the clock, "to prepare, since I expected in a few minutes to come at a stratum of the *nebulæ*, "finding myself already" (as I then figuratively expressed it) "on nebulous ground." In this I succeeded immediately; so that I now can venture to point out several not far distant places, where I shall soon carry my telescope, in expectation of meeting with many *nebulæ*. But how far these circumstances of vacant places preceding and following the nebulous strata, and their being as it were contained in a bed of stars, sparingly scattered between them, may hold good in more distant portions of the heavens, and which I have not yet been able to visit in any regular manner, I ought by no means to hazard a conjecture. The subject is new, and we must attend to observations, and be guided by them, before we form general opinions.

Before I conclude, I may, however, venture to add a few particulars about the direction of some of the capital strata or their branches. The well known nebula of Cancer, visible to the naked eye, is probably one belonging to a certain stratum, in which I suppose it to be so placed as to lie nearest to us. This stratum I shall call that of Cancer. It runs from a Cancrion towards the south over the 67. nebula of the *Connoissance des Temps*, which is a very beautiful and pretty much compressed cluster of stars, easily to be seen by any good telescope, and in which I have observed above 200 stars at once in the field of view.

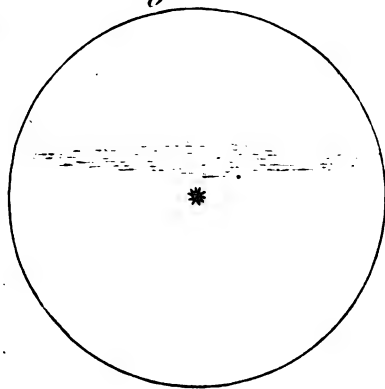
view of my great reflector, with a power of 157. This cluster appearing so plainly, with any good, common telescope, and being so near to the one which may be seen by the naked eye, denotes it to be probably the next in distance to that within the quartile formed by γ , δ , η , θ ; from the 67th nebula the stratum of Cancer proceeds towards the head of Hydra; but I have not yet had time to trace it farther than the equator.

Another stratum, which perhaps approaches nearer to the solar system than any of the rest, and whose situation is nearly at rectangles to the great sidereal stratum in which the sun is placed, is that of Coma Berenices, as I shall call it. I suppose the Coma itself to be one of the clusters in it, and that, on account of its nearness, it appears to be so scattered. It has many capital nebulae very near it; and in all probability this stratum runs on a very considerable way. It may, perhaps, even make the circuit of the heavens, though very likely not in one of the great circles of the sphere: for, unless it should chance to intersect the great sidereal stratum of the milky way before-mentioned, in the very place in which the sun is stationed, such an appearance could hardly be produced. However, if the stratum of Coma Berenices should extend so far as (by taking in the assistance of M. MESSIER's and M. MECHAIN's excellent observations of scattered nebulae, and some detached former observations of my own) I apprehend it may, the direction of it towards the north lies probably, with some windings, through the great Bear onwards to Cassiopeia; thence through the girdle of Andromeda and the northern Fish, proceeding towards Cetus; while towards the south it passes through the Virgin, probably on to the tail of Hydra and the head of Centaurus. But, notwithstanding I have already fully ascertained the existence and direction of this stratum for more
than

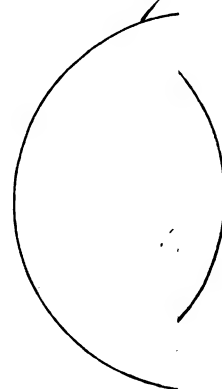
Fig. 1.



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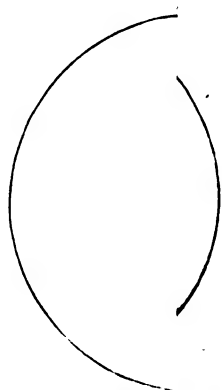
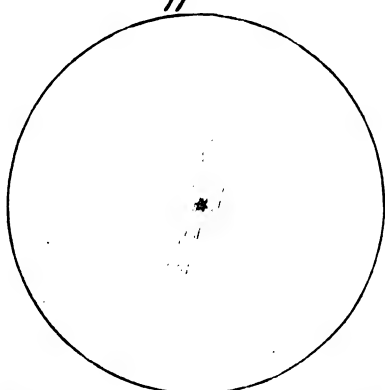
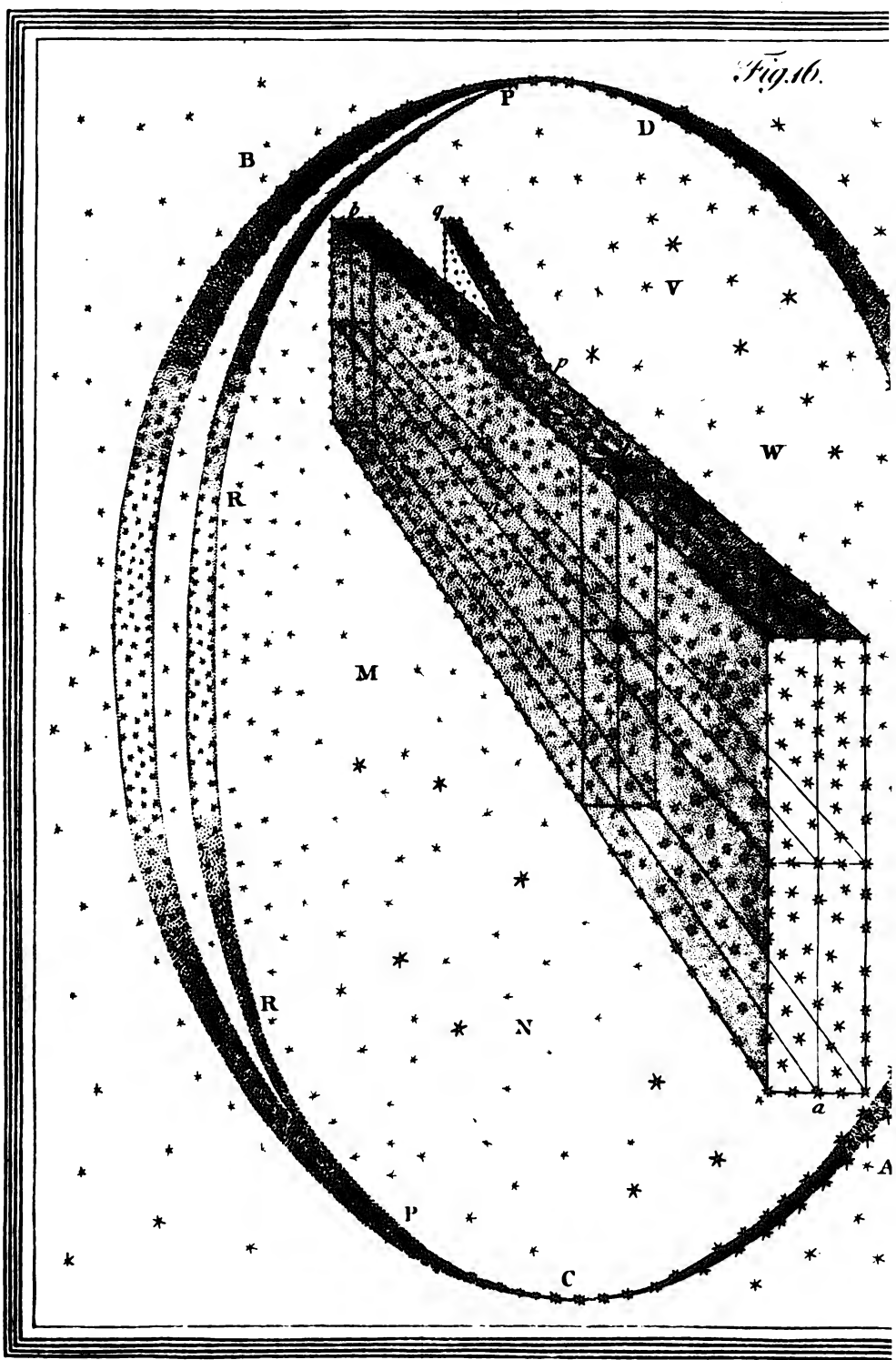


Fig. 16.



than 30 degrees of a great circle, and found it almost every where equally rich in fine nebulæ, it still might be dangerous to proceed in more extensive conjectures, that have as yet no more than a precarious foundation. I shall therefore wait till the observations in which I am at present engaged shall furnish me with proper materials for the disquisition of so new a subject. And though my single endeavours should not succeed in a work that seems to require the joint effort of every astronomer, yet so much we may venture to hope, that, by applying ourselves with all our powers to the improvement of telescopes, which I look upon as yet in their infant state, and turning them with assiduity to the study of the heavens, we shall in time obtain some faint knowledge of, and perhaps be able partly to delineate, *the Interior Construction of the Universe.*

Datchet near Windsor,
April, 1784.

WILLIAM HERSCHEL

XXXIV. *An Account of a new Species of the Bark-Tree, found in the Island of St. Lucia. By Mr. George Davidson; communicated by Donald Monro, M. D. Physician to the Army, F. R. S.*

Read June 24, 1784.

DR. DONALD MONRO.

SIR,

HAVING received from my correspondent Mr. DAVIDSON, surgeon, in the island of St. Lucia, some Bark, the product of that island, which is said to possess the virtues of the Jesuit's Bark, and in a much smaller dose, I shall esteem it a favour if you will lay before the Royal Society the specimen which I have sent to you with this letter, together with Mr. DAVIDSON's account of it, if you think they merit that honour.

I have examined the dried specimens very carefully. They are not so well preserved as I could wish; but I have since seen much finer in the possession of Sir JOSEPH BANKS, who has done me the honour to favour me with the following character, as most distinctive of it from the other species of Cinchona already described, which he gave me an opportunity of examining.

It

It is undoubtedly a *Cinchona*, but not the *Cinchona officinalis* of LINNÆUS; for it differs from it essentially in its bark in several particulars. It has an emetic quality not common to the true bark, breaks more woody and splintery, and is far more nauseous to the taste. Its decoction is of a dull Burgundy colour; and its extract resembles more the bitter of Gentian than that of the Quinquina. I have procured four ounces of it from half a pound of the Bark boiled in water, and herewith send to you a small specimen.

The drawings, which accompany this letter, are exact copies of the specimens which I received; I therefore hope they will not be thought unworthy the acceptance of the Royal Society.

I have the honour to be, &c.

Henrietta-street, Nov. 6, 1783.

G. WILSON.

Botanic character of the Bark-Tree of St. Lucia.

“*Cinchona floribus paniculatis, glabris; laciniis linearibus, tubo longioribus; staminibus exsertis; foliis ellipticis, glabris.*”

Extract of a Letter from Mr. GEORGE DAVIDSON, dated St. Lucia, July 15, 1783.

IT is now about four years since Mr. ALEXANDER ANDERSON discovered in the woods, near the Grand Cul de Sac, some trees resembling, in the botanical characters, the

N n n 2

true

true *Quinquina* of LINNÆUS. He brought the bark, flowers, and seeds, to Dr. YOUNG of the General Hospital, and trial was made of it there; but not being sufficiently dried, its strong emetic and purgative qualities prevented its exhibition.

The publication of Dr. SAUNDERS, which I received about two months ago, mentioning the introduction of a species of bark of a redder colour, and possessing greater powers than the bark formerly in use, induced us here to try the bark of this country. Dr. YOUNG had by him some that was collected in General GRANT's time: on account of the length of time it had been kept, and its being sufficiently dried, he has met with all the success he could wish.

It is manifestly more astringent than the bark, and the bitter is likewise more durable on the palate.

Hitherto I have generally used the cold infusion, either in lime or simple water, in the proportion of one ounce to three pints of the water. I have likewise given it in substance from twenty to thirty grains; but never exceeded the last quantity, for I never found the stomach able to retain more than twenty grains.

Joined with the *Canella alba*, it forms in spirits an agreeable and elegant tincture. I have made a tincture from the seeds, which are infinitely stronger in taste than the bark itself.

(Signed)

GEO. DAVIDSON.

Mr.

Mr. George Davidson's account of the Bark-Tree of the island of St. Lucia.

THE Bark-Tree of this island is nearly about the size of the cherry-tree, seldom thicker than the thigh, and tolerably straight; the wood is light and porous, without any of the bitterness and astringency of the bark itself.

It delights in a shady situation, the north-west aspect of hills, under larger trees; and is generally to be found about the middle of an hill, near some running water.

The leaves are large, oblong, opposite, and plain, preserving (as well as the flowers and seeds) the bitter taste of the bark.

In the beginning of the rainy season (June), the tree puts forth its flowers in small tufts; at first they are white, but afterwards turn purplish. The stamina are five in number, with a single style. The germen is oblong, bilocular, and furrowed on each side. The seeds are many, and of the winged kind. The corolla is monopetalous, with its mouth divided into five long segments.

The soil in general where it grows is a stiff red clay. The bark itself is of a lighter red than that sent out here to the hospital under the name of *red bark*. It inclines more to the colour of cinnamon. The bitterness and astringency appear to be greater than in either of the other barks.

I apprehend, the proper season for obtaining it is about the month of March, before the flowers come out: after-experience will best determine this.

Infused in cold water, in which form, or in lime-water, I generally use it, it forms a very red tincture, possessing the
bitterness

bitterness and astringency of the bark very strongly. A few drops of the Tinctura florum martialium give it a very black colour, and occasion a copious deposition of a black sediment. It does the same with the spirituous tincture.

With spirits it forms a beautiful red tincture.

Explanation of the references tab. XIX.

- A. A branch of the Cinchona of St Lucia, with the flowers not yet opened.
- B. The entire seed-vessels.
- C. A seed-vessel split.
- D. One of the seeds, of its natural size.
- E. The same magnified.

n. 456.



5a/100 J.

XXXV. *An Account of an Observation of the Meteor of August 18, 1783, made on Hewit Common near York. In a Letter from Nathaniel Pigott, Esq. F. R. S. to the Reverend Nevil Maskelyne, D. D. F. R. S. and Astronomer Royal.*

Read June 24, 1784.

REVEREND SIR,

York, Oct. 18, 1783.

ON the 19th of last August I communicated to you an account of the remarkably fine meteor, which I had seen under circumstances peculiarly favourable the preceding night. I was then preparing myself for a journey into the East Riding; and, on that account, obliged to postpone the verifications, mentioned hereafter, till my return.

On the 18th of August, about ten o'clock P.M. after a hot day, the weather a little hazy, but not so as to obliterate the stars, and no wind, being on horseback, in company with two other gentlemen, on Hewit Common, about three miles from York, my attention was attracted towards the W.N.W. by several faint flashes of lightning, such as are often seen near the horizon, or which may be still better compared to flashes of an aurora borealis. Soon after which I perceived some luminous matter in motion, and collecting together from several directions, fig. 1. (tab. XX.) which immediately taking fire presented itself under the form of a ball, of so vivid a brightness, that the whole horizon was illuminated, so that the smallest object might have

2

have been seen on the ground. This ball, when formed, began to move, with an easy sliding motion, from W.N.W. towards the S.S.E. It suggested the idea of a highly brilliant comet, emitting a train or tail, but of a different colour from the ball itself, this last being of a most brilliant bluish white, and the tail of a dusky red, the length of which appeared to extend over fifteen or more degrees of the heavens, fig. 2. The apparent diameter of the nucleus seemed one-third or one-fourth of the full moon's diameter. The greatest difficulty in this estimation hence arises, that I cannot, notwithstanding all my endeavours, represent in my mind the moon otherwise than as a plane or disk; nor the meteor, than as a spherical body. The altitude of it, when it formed in the W.N.W. was about 30° ; and about 19° or 20° above the horizon, when it became extinct in the S.S.E. a few sparks of the tail, nearest the nucleus, scattering themselves much in the same manner as those of a sky-rocket when burnt out, fig. 3.

It has been said, that the ball divided itself into three or four parts before its extinction. To me it appeared to vanish or gently die away: what confirms me in the opinion, that it did not divide, is, that the three or four scattering parts above-mentioned were not of the bright colour of the ball itself, but of the dusky red which the tail invariably shewed. The interval of time from the meteor's formation to its extinction was nearly twenty seconds, perhaps two or three seconds less. The long habit I have of counting seconds in astronomical observations induces me to think this quantity may be relied on; and this I mention, because some have estimated it more, some less. Nine or ten minutes after its dissipation, I heard a noise, much resembling the report of a cannon at a very great distance; but I would not wish to have it understood, that I
 speak

peak to this last interval with the same certainty as to the other; if, however, it be exact, and supposing found to move 1106 feet in one second of time, and the same in the upper regions of the atmosphere as here below, which, however, may be very different, its distance from me, at its extinction, must have been about 120 miles, and its perpendicular altitude above the earth's surface about 40 miles.

I have added a scheme and a small sketch, presuming by that means to convey a clearer idea of what I saw. The altitudes, azimuths, &c. are not merely from estimation. After my return from the East Riding, I went to the very spot, where I had seen the meteor on the 18th of August. The road, as in the scheme, being exactly straight from my station, both *towards* and *from* York, no mistake can arise in that respect. With all the circumstances clearly and forcibly impressed on my mind, I watched till some remarkable spot in the sky presented itself at the same place in which I had seen the meteor itself form, cross the road, vanish, &c.: then, with a theodolite, I took the several bearings, which may be the more relied on, as I repeated the operations three different times, on different spots, which agree surprisingly well for measures where no minute exactness can be expected. I have marked minutes in the scheme, because the results gave them, without any pretension to such nicety.

I am, &c.

NATH. PIGOTT.



XXXVI. *Observations of the Comet of 1783. In a Letter from Edward Pigott, Esq. to the Rev. Nevil Maskelyne, D. D. F. R. S. and Astronomer Royal.*

Read June 24, 1784.

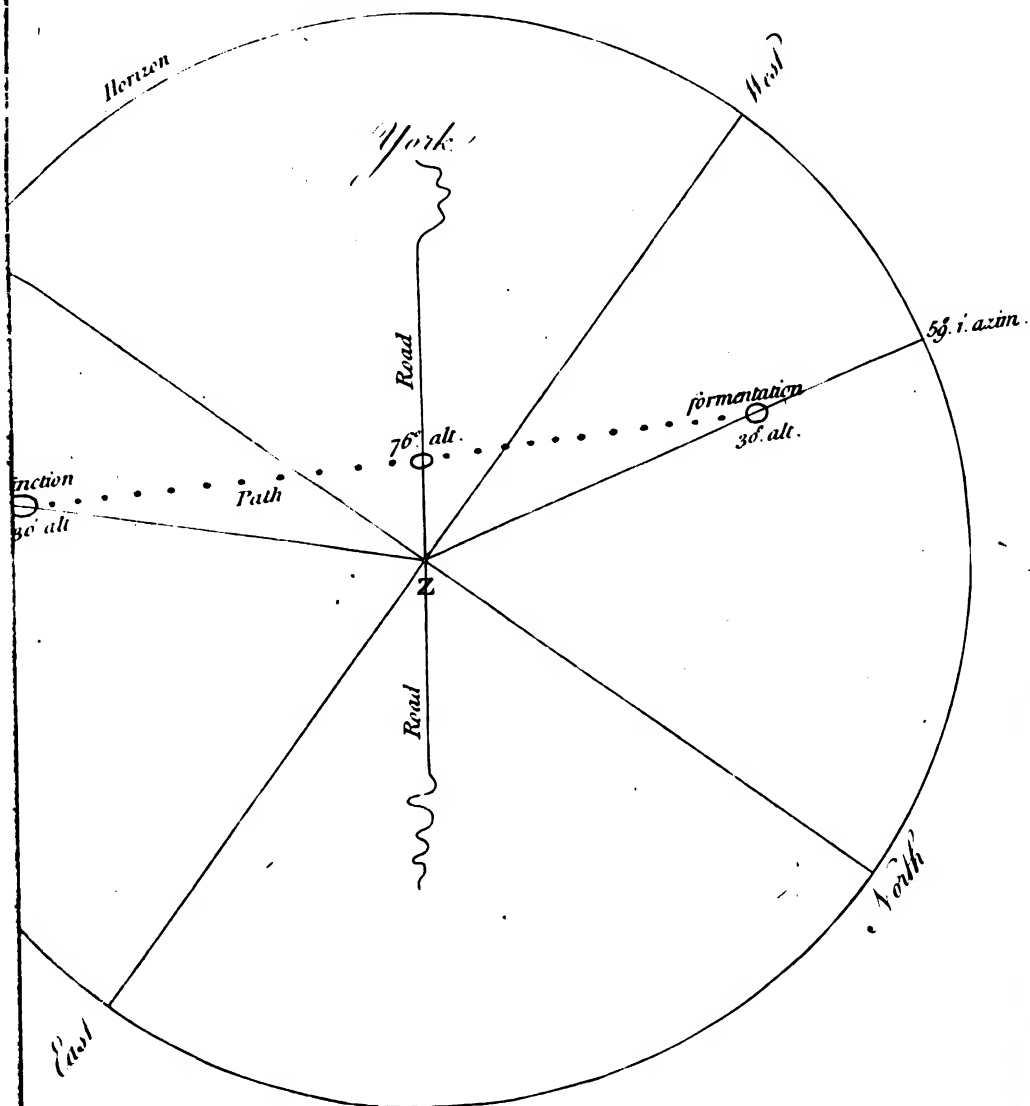
REVEREND SIR,

York, Dec 11, 1783.

HAVING completed my observations of the comet I discovered on the 19th of November last, I take the liberty of desiring you to present them to the Royal Society. The faintness of the comet's light, and the unfavourable sky you have had in the south, induce me to believe, that few observations of it have been made besides the following.

Dates.	Apparent time.	R. A.	North declination.	Greatest error of each R. A.	Longitude.	Latitude.
1783	h. m. s.	° ' "	° ' "	" " "	S. ° ' "	" " "
Novem. 19	11 4	41 1 30	3 11 1	3 00	1 9 37	12 4½ S
20	10 55	40 0 3	4 32 4	0 22	1 9 27	10 31
22	6 52	38 21 10	6 50 1	0 30	1 8 11	7 50
24	10 24	36 29 28	9 36 4	0 15	1 7 19	4 52½
26	10 9	34 49 20	12 3 4	0 15	1 6 33½	2 6
Decem. 3	15 54	29 21 50	20 15 4	0 40	1 4 24	7 42½ N

The R. A's of November 20th, 24th, and 26th, were deduced from observations made at the transit instrument: the others, except the first, were determined with an excellent 2½ feet night-glass, made by DOLLOND, magnifying 20 times, having cross wires at right angles in its focus, which were visible



Bayer. l.

visible without being illuminated. With this instrument the comet, by the common method, was compared to stars in the field of the telescope, and within four minutes on the same parallel. The places of those stars were afterwards settled with the meridian instruments. As sometimes several stars were observed, I easily found to what degree of certainty those observations might be depended on, which I have marked with the above results. The declinations, I think, cannot err two minutes, being compared to stars within four minutes on the same parallel. The three of November 20th, 24th, and 26th, were taken with the transit instrument by comparing the comet to the nearest stars. I was much chagrined in not being able to see the comet in our equatorial when the wires were illuminated.

The comet had exactly the appearance of a nebula: its light was so faint that it could not be seen in a good opera glass. In the night-telescope the nucleus was scarcely visible, and the diameter of the surrounding coma was about three minutes of a degree. Between the 19th and 26th of November, I thought it had rather diminished in brightness. December the 1st and 3d it was very difficult to be seen, occasioned perhaps by its little elevation above the horizon. Between December the 3d and 10th, the comet was entirely effaced by the increased light of the Moon. On the 10th, the moon being in the horizon did not obliterate stars of the eighth or ninth magnitude; but I could not find the comet. The following observations were made by my friend Mr. JOHN GOODRICKE.

462 *Mr. P. GOTT's Observations of the Comet of 1783.*

Dates.	Apparent time.	R. A.	North declination.	Longitude.	Latitude.
1783	h. ' "	° ' "	° ' "	S. ° ' "	° ' "
Novem. 24	8 16	36 32 57	9 30½	1 7 21	1 42½ S
28	6 8½	33 20 0	14 16½	1 5 55½	0 52½ N

I am, &c.

EDW. PIGOTT.

P. S. This morning I received a letter from M. DE MECHAIN, in which he informs me, that he discovered the comet on the 26th of November seven days after my first observation. He has made several observations on it.

XXXVII. *Experiments on mixing Gold with Tin. In a Letter from Mr. Stanesby Alchorne, of his Majesty's Mint, to Peter Woulfe, Esq. F. R. S.*

Read June 24, 1784.

DEAR SIR,

Tower of London,
March 25, 1784.

YOU know it is a generally received opinion among metallurgists, that tin has a property of destroying the ductility of gold, on being melted with it, even in very small quantities. Our late ingenious countryman Dr. LEWIS, in his *Philosophical Commerce of Arts*, p. 85. has well expressed the sense of most writers on this subject, in the following words: "The most minute proportion of tin and lead," says he, "and even the vapours which rise from them in the fire, though not sufficient to add to the gold any weight sensible in the tenderest balance, make it so brittle that it flies in pieces under the hammer."

Divers circumstances, nevertheless, long since induced me to disbelieve the fact; but these, having chiefly arisen from small experiments, did not seem to warrant any general conclusion. A late public occasion, however, which led me to various trials of mixing these metals together, in different proportions, and in sufficiently large quantities, has put the matter out of dispute; and shewn me, that tin, in small quantity at least, may be added to gold, either pure or alloyed, without producing any other effect than what might easily be conceived,

ceived, *à priori*, from the different texture of the two metals. In confirmation of which, I beg leave to lay some of the experiments before you.

EXPERIMENT I.

Sixty Troy grains of pure tin were stirred into twelve ounces of refined gold, in fusion; and the mixture was then cast into a mould of sand, producing a flat bar, one inch wide, and one-eighth of an inch thick. The bar appeared sound and good, suffered flattening under the hammer, drawing several times between a large pair of steel rollers, and cutting into circular pieces, of near an inch diameter, which bore stamping in the money-press, by the usual stroke, without shewing the least sign of brittleness; or rather with much the same ductility as pure gold.

EXPERIMENT II.

Ninety grains of like tin were added to twelve ounces of fine gold, stirred, and cast as above. The bar produced was scarcely distinguishable from the former, and bore all the operations, as before-mentioned, quite as well.

EXPERIMENT III.

One hundred and twenty grains of fine tin were mixed with twelve ounces of fine gold, and being cast like the foregoing, produced a bar rather paler and harder than the preceding, but which suffered the like operations very well; except that, on drawing between rollers, the outer edges were disposed to crack a little.

EXPE-

E X P E R I M E N T I V.

One hundred and forty grains, or half an ounce, of the like grained tin, were mixed, as before, with twelve ounces of fine gold; and the bar resulting from this mixture was completely sound and good; evidently paler and harder, however, than any of the foregoing, and cracking rather more than the last on passing between the rollers; but bearing every other operation, even stamping under the press, by the common force, without any apparent injury.

E X P E R I M E N T V.

One ounce of tin was next stirred into twelve ounces of the like refined gold, and then cast as before; but the bar produced, though seemingly solid and good, was bad coloured, brittle in texture, and, on the first passing between the rollers, split into several pieces, so that no farther trials were made with it.

E X P E R I M E N T V I.

To inquire how far the fumes of tin, brought into contact with the gold, would do more than mixing the metal in substance, a small crucible, filled with twelve ounces of standard gold, $\frac{1}{11}$ fine, was placed in a larger crucible, having one ounce of melted tin in it; and kept there in fusion, the whole being covered by another large inverted crucible, for about half an hour. In this time a full quarter part of the tin was calcined; but the gold remained unaltered, and equally capable of being manufactured as another portion of the same gold melted in the common manner.

It

It may well be asked, whether the tin, or part of it, in every trial, might not be destroyed, and thus render the conclusions fallacious? But as, in any of these experiments, not more than six or eight grains of the original weight were missing after the casting, and as even fine gold can scarcely be melted without some loss in the operation, so we may reasonably suppose, that our small losses, in the foregoing trials, do not deserve consideration.

The above experiments then seem to shew, that tin is not so mischievous to gold as hath been generally represented. But it would be unfair to infer, that the original author of this doctrine (from whom so many have implicitly transcribed) had no foundation for the assertion. Gold and Tin, indeed, are substances pretty well known; but it is easy to imagine, that coins or trinkets may have been used for one, and impure tin, or pewter, perhaps, for the other; and it is difficult to guess what might be the result of such uncertain combinations. To inquire farther, therefore, the experiments were continued as follows.

EXPERIMENT VII.

To determine whether the two metals might be more intimately combined, and the mass rendered brittle, by additional heat; the mixture of gold and tin, produced in the first of these experiments, was re-melted in a stronger fire than before, and thus kept in fusion full half an hour. By this operation six grains only were lost in the weight; and the bar obtained was no less manufacturable than at first.

EXPE-

EXPERIMENTS VIII. AND IX.

The mixtures of gold and tin, from the second and fourth experiments, were re-melted separately, and one ounce of copper added to each. Being both well stirred, they were cast as usual; and the bars, though sensibly harder, bore all the operations of manufacturing as before. The last bar cracked a little at the edges, on drawing through the rollers, as it had done without the copper, but not materially, and bore cutting rather better than in its former state.

EXPERIMENTS X. AND XI.

A quarter of an ounce of the last mixture (being tin half an ounce, and copper one ounce, with gold twelve ounces), and as much of the bar from experiment the third (being tin one hundred and twenty grains with gold twelve ounces), were each melted by a Jeweller, in the most ordinary manner, with a common sea-coal fire, into small buttons, without any loss of weight. These buttons were forged by him into small bars, nealing them often by the flame of a lamp, and afterwards drawn each about twenty times through the apertures of a steel plate, into fine wire, with as much ease as coarse gold commonly passes the like operation.

EXPERIMENT XII.

To enquire whether the adding of tin to gold, already alloyed, would cause any difference, sixty grains of tin were stirred into twelve ounces of standard gold, $\frac{1}{11}$ fine; and the result passed every operation before described, without shewing the least alteration from the tin.

For greater certainty, several other trials were made, of different mixtures of copper, tin, and silver, with gold, even so

low as two ounces and a-half of copper, with half an ounce of tin, to twelve ounces of gold. But these are not worth particularizing; for they all bore hammering, and flattening by rollers, to the thinness of stiff paper, and afterwards working into watch-cases, cane-heads, &c. with great ease. They all, indeed, grew more hard and harsh, in proportion to the quantity of alloy; but not one of them had the appearance of what all workmen well know by the name of brittle gold. Whence it should seem, that neither tin in substance, or the fumes of it, tend much to render gold unmanufacturable.

Whenever, therefore, brittleness has followed the adding small quantities of tin to fine gold, it must be supposed to have arisen from some unfriendly mixture in the tin, probably from Arsenic; for other experiments have shewn me, that twelve grains of regulus of arsenic, injected into as many ounces of fine gold, will render it totally unmalleable.

From the foregoing experiments, I presume, we may fairly conclude, that though tin, like other inferior metals, will contaminate gold, in proportion to the quantity mixed with it, yet there does not appear any thing in it specifically inimical to this precious metal. And this being contrary to the doctrine of most chemical writers, I submit to your better judgement, whether it may not be useful to publish these experiments, by laying them before the Royal Society.

I am, &c.

S. ALCHORNE.



XXXVIII. Sur un moyen de donner la Direction aux Machines Aérostatiques. Par M. Le Comte De Galvez. Communicated by Sir Joseph Banks, Bart. P. R. S.

Read July 1, 1784.

NOUS souffignés certifions, que M. le Comte DE GALVEZ nous ayant communiqué ses idées sur le moyen de pouvoir donner la direction aux machines aérostatiques, pour faire route à volonté et par un rumb certain et assuré dans l'air, principalement fondé sur différentes observations qu'il avoit faites sur l'usage que les oiseaux font de leurs aîles quand ils volent, et sur celui que font les poissons de leurs nageoires et de leur queue quand ils nagent :

Nous nous sommes transportés, l'après-midi du premier Mars de cette année 1784, au canal de Manzanarés, où on avoit préparée une chaloupe de vingt-cinq pieds de long sur quatre et demi de large, avec une machine qu'il avoit inventée pour démontrer ses idées. Cette machine *, qui consistoit en un chevalet qui alloit de poupe à proue à la hauteur de cinq pieds, étoit croisée en rectangles par trois vergues de bois élastique, de dix-huits pieds de long chacune, avec une aîle à chaque bout, composée de baguettes de baleine, couvertes d'un morceau de taffetas de cinq pieds de long, et trois de large, laquelle étoit jointe par un de ses quatre côtés à la vergue, de façon que l'aîle restoit horizontale. Le mouvement se communiquoit à chaque vergue, et par conséquent à ses deux aîles, par un seul homme, qui tirant avec vitesse des cordes attachées aux bouts de chaque vergue, les agitoit verticalement, d'où resultoit que

* See tab. XXI. fig. 1.

quand elles se plioient, les aîles prenoient à leurs extrémités une inclination de quarante-cinq degrés de l'horison. Ce mouvement et celui de la réaction produisoient dans la chaloupe, où il y avoit six hommes, une impulsion qui la faisoit marcher contre le courant du canal et le peu d'air qu'il faisoit, cent cinquante pieds par minute, outre soixante pieds qu'elle parcouroit avant de s'arrêter depuis l'instant qu'on cessoit de mouvoir les aîles : elle parcouroit deux cents quarante-trois pieds par minute, allant avec le courant et l'air, par le même mouvement continu des aîles.

Nous fîmes tous très-étonnés de l'effet que produisit cette expérience ; car, quoique le desir qu'avoit l'inventeur de mettre ses idées en pratique au plutôt, fut cause qu'il se servit d'une chaloupe lourde et mal construite, avec laquelle les aîles n'avoient point de proportion ; nous sommes persuadés que la situation des aîles et leur mouvement vertical, qui formoient lors qu'on les batoient un plan incliné, imitant en cela les oiseaux et les poissons, fournissent un principe sûr et certain pour donner une direction par quelque rumb que se soit, à toute espèce de corps qui nagent dans un fluide, et par conséquent très-applicable aux nouvelles machines aërostatiques.

Cette invention nous paroît digne de l'approbation et de l'éloge des physiciens qui, sans doute, employeront leurs efforts pour lui donner toute la perfection dont elle est susceptible dans l'exécution de son mécanisme.

Et pour constater que la dite expérience a été faite de la manière qu'on vient d'exposer, nous avons signé la présente certification, ainsi qu'un dessein de la dite machine, à Madrid le deux Mars, mil sept cent quatre-vingt-quatre. D. JOSEF DE VIEXA, D. AGUSTIN BETANCOURT Y MOLINA, D. RICARDO WORSLEY, RAIM DE S. LAURENT, CASIMIRE ORTEGA.



XXXIX. *An extraordinary Case of a Dropsy of the Ovarium, with some Remarks. By Mr. Philip Meadows Martineau, Surgeon to the Norfolk and Norwich Hospital; communicated by John Hunter, Esq. F. R. S.*

Read July 1, 1784.

SARAH KIPPUS, a pauper in the city of Norwich, was, for many years, a patient of my father's, and, at his decease, was under the care of Mr. SCOTT, as city surgeon, who obliged me many times by taking me to the poor woman, from whom I received the account of the early part of her disease.

Her complaints came on first after a miscarriage at the age of 27. She had never been pregnant before; and her discharges at that time were so great as to bring her into a very weak condition. She soon perceived some uneasiness, attended with a swelling, on one side, which, after a few months, became too large to distinguish whether it was greater on one side or the other. As the swelling was found to arise from water, it was drawn off, which was in the year 1757. She was never afterwards pregnant; but the catamenia continued regularly till the usual period of their cessation. When I first saw her, which was in the year 1780, she had been many times tapped, and she was then full of water. Her appearance was truly deplorable, not to say shocking. She was rather a low woman, and her body so large as almost wholly to obscure her face, as well as every other part of her: with all she was tolerably

rably chearful, and seldom regarded the operation. I saw her just before we took away 106 pints of water, and I begged leave to take a measure of her. She was sixty-seven inches and a half in circumference, and from the cartilago enfiformis to the os pubis thirty-four inches. Her legs were now greatly swelled; but this, and every other symptom of which she complained, evidently arose from the quantity and weight of water. She neither ate nor drank much, and made but a small quantity of urine.

The operation of drawing off the water was generally performed on a Sunday, as the most convenient day for her neighbours to assist her, and before the latter end of the week she was able to walk very well. She was first tapped in the year 1757, and died in August 1783. Thus she lived full twenty-five years with some intervals of ease, having eighty times undergone the operation, and in all had taken from her 6631 pints of water, or upwards of thirteen hogheads.

I will subjoin the account of the dates, and the quantity drawn off at each time, as given me by Mr. SCOTT, observing that till 1769 no exact memorandum was kept, except of the *number* of times, although the quantity of water drawn off was always measured. By my father she was tapped twenty-six times, averaged at 70 pints each time: by Mr. DONNE once, 73 pints, which makes 1683 pints from some part of the year 1757 to 1769. By Mr. SCOTT as follows:

1769.

1769. Pints.
Mar. 16. 70
July 17. 72
Nov. 20. 78
Dec. 31. 70
—
290

1770.
April 15. 70
Aug. 11. 73
Dec. 4. 76
—
219

1771.
Mar. 22. 74
July 14. 78
Nov. 3. 79
—
231

1772.
Feb. 22. 79
June 6. 73
Sept. 12. 74
Dec. 12. 82
—
308

1773.
March 7. 78
May 29. 71
Aug. 29. 79
Dec. 5. 81
—
309

1774. Pints.
Mar. 13. 77
June 26. 89
Oct. 23. 92
—
258

1775.
Jan. 24. 94
May 28. 91
Sept. 13. 72
Dec. 16. 80
—
337

1776.
April 9. 84
July 28. 82
Nov. 27. 85
—
251

1777.
Mar. 16. 89
July 27. 90
Nov. 9. 98
—
277

1778.
March 8. 96
July 5. 99
Nov. 5. 105
—
300

1779. Pints.
Feb. 28. 106
June 13. 108
Aug. 17. 92
Oct. 24. 99
Dec. 10. 90
—
495

1780.
Feb. 6. 73
Apr. 23. 102
July 24. 106
Sept. 10. 95
Nov. 12. 98
—
474

1781.
Jan. 1. 100
Mar. 11. 94
June 25. 100
Oct. 14. 100
—
394

1782.
Jan. 13. 99
Mar. 18. 64
June 2. 74
Aug. 25. 98
Nov. 17. 90
—
425

	Pints.
1783.	
Feb. 14.	104
May 11.	100
July 20.	98
Aug. 11 on opening	78
	<hr/>
	380
Total	6631 pints.

In looking over this account it appears, that 108 pints was the greatest quantity ever taken away at any one time; that she was never tapped more than five times in any one year; and the largest quantity in a year was 495 pints. The most collected in the shortest space of time was 95 pints in seven weeks, from July 24th to September 10th in 1780, which is very nearly two pints a day. It appears also, that in the last 14 years of her life, when a regular account was kept, she increased faster in the winter than in the summer months. If the six summer months from April to September inclusive are reckoned, she lost in the 14 years in 23 operations 1972 pints, and in the winter months from October to March inclusive, by 30 tapplings, 2596 pints; and it will be found, that 30 is to 2596 rather more than 23 to 1972, so that seven more tapplings were at least necessary in the winter than in the summer. In the months of March and November she oftener underwent the operation than in any other. In these calculations the three months in 1783 are not included, as the year was not finished.

If we compare the famous case of Lady PAGE, related by Dr. MEAD, the quantity of water taken from her ladyship appears small when opposed to the number of pints drawn from

from SARAH KIPPUS. The one lost 1920, the other 6631. It must be confessed, however, that Lady PAGE collected faster than the poor woman whose case I have related.

I come now to speak of the dissection, and to make some observations on the whole. On the 10th of August 1783, the poor woman died; and the following day Dr. DACK, an eminent physician of this place, accompanied me to open the body. I first drew off 78 pints of clear water: supposing, therefore, all the water to have been taken away at the last operation, then in three weeks she had collected 78 pints, which is more than three pints and a half in each day: a quantity far exceeding what she had taken. I then opened into the cavity from which the water came, and separated the sac from the peritoneum, and found the sac had arisen in the ovarium of the left side. After this, I dissected out the uterus, with the right ovarium in a natural state, and thus obtained every part necessary to show the disease, *viz.* the uterus, the right ovarium sound, and the left enlarged into an immense pouch. The cyst itself was not very thick, but lined in almost every part of it, but more especially in the fore part, with small ossifications. The peritoneum was *prodigiously* thickened, and thus, by its additional strength, became the chief support of the water. There was something singular in the sac itself, for it was rather two than one, from there being an opening in the side of what appeared at first the only cavity, which led to another cavity, almost equally large with the first, so that if all the water in any operation had not been evacuated, it must probably have been owing to a difficulty in its passage from the second into the first or more external cyst. From the size, however, of the poor woman after each operation, it is evident, that in her there being two sacs did not prevent the total drawing off of

the water. The other viscera appeared all in a natural state. The intestines were quite empty, and pushed up under the ribs, so as to have left but very little room for the expansion of the lungs within the thorax. The bladder was contracted, or rather I should say appeared lessened. The kidneys were healthy, and both ureters in a natural state. The sac is in the collection of JOHN HUNTER, esq.

In reflecting upon this case, an obvious question arises; from whence proceeded this immense collection of water? At different periods of this poor woman's life the quantity drawn off, without considering the urine she made, was much greater than the fluids she drank, which appeared from measuring whatever she took. It appears then pretty certain, that this superabundant quantity must have been taken into the body by absorption; and if we allow the bodies of animals to have this power of absorbing, which we very well know vegetables are possessed of, it will account for many appearances in the animal œconomy. This poor woman collected faster in the wet moist months of winter, than in summer.

From all, this happy conclusion may be drawn, that although human art is at present insufficient to the perfect cure of diseases similar to the poor woman's case I have related, yet nature is continually defending herself from sudden death; and such relief *may* be granted as to protract life a long time without much pain, and often with intervals of great ease and comfort.



XL. *Methodus inveniendi Lineas Curvas ex proprietatibus Variationis Curvaturæ. Auctore Nicolao Landerbeck, Mathes. Profess. in Acad. Upsalienfi Adjuncto. Communicated by Nevil Maskelyne, D. D. F. R. S. and Astronomer Royal.*

Read July 1, 1784.

P A R S S E C U N D A *.

CURVAS, ex proprietate variationis curvaturæ invenire, indice per functionem coordinatarum cujusdam expresso, problema etsi indeterminatum est; juvat tamen ad curvas cognoscendas, quum facile et sponte sese offerunt conditiones determinantes qui rei conveniunt et quæ in casu quovis examini subiecto locum habent. Quo consilio et qua arte calculum inire oporteat, ut et hæc et his affinia peragenda sint, quæ ad curvas ex curvaturæ variatione cognoscendas pertineant, per theoremata quæ sequuntur, exponere conabor.

T H E O R E M A I. (Vide tab. XXI. fig. 2.)

Si curvæ cujusdam LC index variationis curvaturæ sit T, radius curvedinis R, sinus anguli BCD p , posito sinu toto 1, arcus curvæ LC \propto coordinatæ perpendiculares x et y earumque fluxiones dp , dz , dx , et dy dicantur, erit $\frac{dz}{\int T dz} = -\frac{dp}{\sqrt{1-p^2}}$.

Quoniam $dx = -Rdp$ et $dz = -\frac{dx}{\sqrt{1-p^2}}$ habetur $\frac{dz}{R} = -\frac{dp}{\sqrt{1-p^2}}$

* See Vol. LXXIII. p. 456.

Q q q 2

et

et quum $dR = Tdz$ erit $R = \int Tdz$ et substitutione $\frac{dz}{\int Tdz} = -\frac{dp}{\sqrt{1-p^2}}$.

Cor. 1. Hinc obtinetur $\frac{dx}{R} = -dp$, $\frac{dy}{R} = -\frac{pdp}{\sqrt{1-p^2}}$ et $\frac{dz}{R} = -\frac{dp}{\sqrt{1-p^2}}$.

Cor. 2. Si Tangens anguli BCD per r , Secans per s designentur habetur $\frac{dz}{\int Tdz} = -\frac{dr}{1+r^2}$ et $\frac{dz}{\int Tdz} = -\frac{ds}{s\sqrt{s^2-1}}$.

Schol. 1. Ex hoc theoremate facilis deducitur methodus generaliter calculandi variationem curvaturæ curvæ cujuscumque.

Nam $\int Tdz = -\frac{dz\sqrt{1-p^2}}{dp}$, quantitas vero $\frac{dz\sqrt{1-p^2}}{dp}$ datur, data

inter x et y relatione. Sit valor quantitatis $-\frac{dz\sqrt{1-p^2}}{dp} = Z$ func-

tioni curvæ z , $\int Tdz = Z$ et sumtis fluxionibus $Tdz = \dot{Z}dz$ qua

$T = \dot{Z}$ functioni ipsius z . Si valor quantitatis $-\frac{dz\sqrt{1-p^2}}{dp} = P$ per

p expressus, erit $\int Tdz = P$ sumtis fluxionibus $Tdz = \dot{P}dp$ et

$T = \frac{\dot{P}dp}{dz}$, quæ functio est quantitatis p , in potestate semper est

$\frac{dp}{dz}$ per p exprimere.

Schol. 2. Hujus etiam theorematibus subsidio inveniri possunt curvæ ex data relatione inter T et z , R et z , R et y , et R et p .

Si enim sit $T = Z$ functioni quantitatis z , erit $\int Tdz = \int Zdz + A$,

vi theorematibus $\frac{dz}{\int Zdz + A} (= \frac{dz}{\int Tdz}) = -\frac{dp}{\sqrt{1-p^2}}$ et integratione

$\int \frac{dz}{\int Zdz + A} + C = -\frac{dp}{\sqrt{1-p^2}}$. Posita $\int \frac{dz}{\int Zdz + A} + C = b$ et N nu-

merus

merus cujus logarithmus hyperbolicus 1 habetur $\sqrt{1-p^2} = \frac{N^{b\sqrt{-1}} - N^{-b\sqrt{-1}}}{2\sqrt{-1}}$ et $p = \frac{N^{b\sqrt{-1}} + N^{-b\sqrt{-1}}}{2}$, quæ functiones sunt quantitatis z , quibus positis Z et $\sqrt{1-Z^2}$ respective proveniunt $x (= \int dz \sqrt{1-p^2}) = \int Z dz$ et $y (= \int p dz) = \int dz \sqrt{1-Z^2}$ quarum alterutra curvarum indoles innotescit.

Si $R = X$ functioni abscissæ x provenit $\frac{dx}{X} (= \frac{dx}{R}) = -dp$ et integratione $\dot{X} (= C - \int \frac{dx}{R}) = p$ unde $\sqrt{1-p^2} = \sqrt{1-\dot{X}^2}$ et $y (= \int \frac{p dx}{\sqrt{1-p^2}}) = \int \frac{\dot{X} dx}{\sqrt{1-\dot{X}^2}}$ æquatio curvæ indolem exprimens.

Et si $R = Y$ functioni ordinatæ y , habetur $\frac{dy}{Y} (= \frac{dy}{R}) = -\frac{p dp}{\sqrt{1-p^2}}$ et integratione $\dot{Y} (= \int \frac{dy}{Y} + C) = \sqrt{1-p^2}$, unde $p = \sqrt{1-\dot{Y}^2}$ et $x (= \int \frac{dy \sqrt{1-p^2}}{p}) = \int \frac{\dot{Y} dy}{\sqrt{1-\dot{Y}^2}}$ quæ exprimit naturam curvæ.

Hinc colligitur quod quoties $T dz$ perfecte integretur et $\int \frac{dz}{\int Z dz + A}$ obtineatur per arcus circulares dum aut $\int Z dz$ aut $\int dz \sqrt{1-Z^2}$ absolutam admittat integrationem, curvæ erunt rectificabiles, et algebraicæ, si relatio inter x et z vel inter y et z in relationem algebraicam inter x et y permutari possit.

Evidens etiam est quod si X functio est algebraica quantitatis x vel Y quantitatis y , et non solum $\frac{dx}{X}$ vel $\frac{dy}{Y}$ sed etiam $\frac{\dot{X} dx}{\sqrt{1-\dot{X}^2}}$ vel $\frac{\dot{Y} dy}{\sqrt{1-\dot{Y}^2}}$ quantitates perfecte integrabiles, curvæ evadunt algebraicæ, alias transcendentes.

Exempl.

Exempl. 1. Inveniendæ fit curva ubi variatio curvaturæ $T = \frac{3 \cdot \sqrt{8a+27z}^{\frac{2}{3}} - za^{\frac{2}{3}}}{a^{\frac{1}{3}} \sqrt{8a+27z}^{\frac{2}{3}} - 4a^{\frac{2}{3}}}$. Ut simplicior reddatur calculus ponatur $\sqrt{8a+27z}^{\frac{2}{3}} = u$ et $a^{\frac{2}{3}} = b$ erit $z = \frac{u^{\frac{3}{2}} - 8b^{\frac{3}{2}}}{27}$, $dz = \frac{du\sqrt{u}}{18}$, $T = \frac{3u - 2b}{\sqrt{b}\sqrt{u-4b}}$ et $\int T dz = \frac{u\sqrt{u}\sqrt{u-4b}}{18\sqrt{b}} + A$; fit constans hæc $A = a$, quod accidit evanescente $\int T dz u = b$, habetur per theorema $\frac{du\sqrt{b}}{u\sqrt{u-4b}} (= \frac{dz}{\int T dz}) = -\frac{dp}{\sqrt{1-p^2}}$ et integratione $\int \frac{du\sqrt{b}}{u\sqrt{u-4b}} + C = -\int \frac{dp}{\sqrt{1-p^2}}$, cujus æquationis termini quum sint arcus circulares quorum finus $\sqrt{1-p^2} = \frac{\sqrt{u-4b}}{\sqrt{u}}$ et cosinus $p = \frac{2\sqrt{b}}{\sqrt{u}}$, posito arcu constanti $C = 0$, obtinetur $y (= \int p dz) = \int \frac{du\sqrt{b}}{9} + B = \frac{\sqrt{u-4b}\sqrt{b}}{9}$ nam $B = \frac{4b\sqrt{b}}{9}$, posita $y = 0$ et $u = 4b$, atque $x (= \int dz\sqrt{1-p^2}) = \int \frac{du\sqrt{u-4b}}{18} = \frac{u-4b}{27}$ quibus æquationibus exterminata u et substituta a habetur $y^3 = ax^2$ æquatio pro parabola cubicâ.

Exempl. 2. Si fit variatio curvaturæ $T = \frac{2z}{a}$ erit $\int T dz (= \int \frac{2z dz}{a}) = \frac{z^2}{a} + A$ et si $Z = 0$ $\int T dz = a$ erit constans $A = a$, atque vi theorematum $\frac{adz}{a^2+z^2} (= \frac{dz}{\int T dz}) = -\frac{dp}{\sqrt{1-p^2}}$ et integratione $\int \frac{adz}{a^2+z^2} + C = -\int \frac{dp}{\sqrt{1-p^2}}$; posito arcu constanti $C = 0$ cæteri sunt æquales eorumque finus et cosinus, unde $\sqrt{1-p^2} = \frac{z}{\sqrt{a^2+z^2}}$, $p = \frac{a}{\sqrt{a^2+z^2}}$ et $dx (= dz\sqrt{1-p^2}) = \frac{z dz}{\sqrt{a^2+z^2}}$ et $dy (= p dz)$

$p dz) = \frac{adz}{\sqrt{a^2 + z^2}}$, quibus constat curvam esse catenariam.

Exempl. 3. Sit variatio curvaturæ $T = \frac{a-z}{\sqrt{2az-z^2}}$, evadit $\int T dz$
 $= \sqrt{2az-z^2}$, per theorema $\frac{dz}{\sqrt{2az-z^2}} (= \frac{dz}{\int T dz}) = -\frac{dp}{\sqrt{1-p^2}}$ et
 per integrationem $\int \frac{dz}{\sqrt{2az-z^2}} + C = -\int \frac{dp}{\sqrt{1-p^2}}$, si arcus ille
 constans $C = 0$, cæteri sunt æquales eorumque sinus et cosinus,
 quo $\sqrt{1-p^2} = \frac{\sqrt{2az-z^2}}{a}$, $p = \frac{a-z}{a}$ et $y (= \int p dz) = \int \frac{a-z}{a} dz =$
 $\frac{2az-z^2}{a}$ æquatio pro cycloide ordinaria.

THEOREMA II.

Manentibus antea adhibitis denominationibus erit $\frac{dx}{y + \int T dx}$

$$= -\frac{dp}{\sqrt{1-p^2}}.$$

Quoniam $\frac{dx}{R} = -dp$, erit dividendo per $\sqrt{1-p^2}$, $\frac{dx}{R\sqrt{1-p^2}}$
 $= -\frac{dp}{\sqrt{1-p^2}}$. Propter $1 : \sqrt{1-p^2} :: CD(R) : CF = R\sqrt{1-p^2}$,
 sed $dz : dx :: T dz : T dx$, quæ fluxio est ipsius DE, quare
 $DE = \int T dx$, unde $CF = y + \int T dx$ qua pro $R\sqrt{1-p^2}$ substi-
 tuta, prodit $\frac{dx}{y + \int T dx} = -\frac{dp}{\sqrt{1-p^2}}$.

Cor. 1. Quantitas $dy + T dx$ semper est perfecte integrabilis.

Nam $T dx = -\frac{ddx\sqrt{1-p^2}}{dp}$ et $dy = \frac{p dx}{\sqrt{1-p^2}}$ unde $dy + T dx = \frac{p dx}{\sqrt{1-p^2}}$
 $= \frac{ddx\sqrt{1-p^2}}{dp}$ et integratione $y + \int T dx = -\frac{dx\sqrt{1-p^2}}{dp}$.

Cor.

Cor. 2. Dicatur femichorda curvaturæ CF F, obtinetur $\frac{dx}{F} = -\frac{dp}{\sqrt{1-p^2}}$, $\frac{dy}{F} = -\frac{pdp}{1-p^2}$ et $\frac{dx}{F} = -\frac{dp}{1-p^2}$.

Cor. 3. Si Tangens anguli BCD per r , Secans per s designentur habetur $\frac{dx}{y + \int T dx} = -\frac{dr}{1+r^2}$ et $\frac{dx}{y + \int T dx} = -\frac{ds}{s\sqrt{s^2-1}}$.

Schol. 1. Per hoc theorema via etiam patet calculandi generaliter variationem curvaturæ. Est enim $y + \int T dx = -\frac{dx\sqrt{1-p^2}}{dp}$, quantitas vero $\frac{dx\sqrt{1-p^2}}{dp}$ datur data inter x et p relatione. Sit valor quantitatis $-\frac{dx\sqrt{1-p^2}}{dp} = X$ functioni abscissæ x æquatione ad curvam inventus, erit $\int T dx = X - y$ et sumtis fluxionibus $T dx = \dot{X} dx - dy$, qua $T = \dot{X} - \frac{dy}{dx}$ ubi tam \dot{X} quam $\frac{dy}{dx}$ sunt functiones abscissæ x . Si valor quantitatis $-\frac{dx\sqrt{1-p^2}}{dp} = P$ per p expressus, erit $\int T dx = P - y$ sumtisque fluxionibus $T dx = \dot{P} dp - dy$, qua $T = \frac{\dot{P} dp}{dx} - \frac{p}{\sqrt{1-p^2}}$ ubi $\frac{\dot{P} dp}{dx}$ functio est quantitatis p , nam $\frac{dp}{dx}$ per p exprimi potest.

Schol. 2. Hoc adhibito theoremate inveniri etiam possunt curvæ, ex data relatione inter T et x , F et x , F et y , F et z , et F et p . Posita enim T functione quantitatis x , patet per curvarum quadraturas, aut perfectam aut imperfectam quantitatis $T dx$ obtineri integrationem. Sit $\int T dx = \dot{X} + \int \ddot{X} dx$ functioni vel algebraicæ vel ex parte transcendenti ipsius x , cujus terminis homogeneous valor ipsius $y = \int \ddot{X} dx$ capiatur,isque ejus indolis ut $\int \ddot{X} + \ddot{X} dx$, vel quod idem est $y + \int T dx =$

$X + \int \frac{dx}{X + \sqrt{X^2 - 1}}$ integratione absoluta habeatur, permanente $Tdx = Tdx \sqrt{1 - X^2}$ perfecte integrabili. Per theorema deinde habetur $\frac{dx}{X + \int \frac{dx}{X + \sqrt{X^2 - 1}}} (= \frac{dx}{y + \int Tdx}) = -\frac{dp}{\sqrt{1 - p^2}}$, et per integrationem $\int \frac{dx}{X + \int \frac{dx}{X + \sqrt{X^2 - 1}}} + C = -\int \frac{dp}{\sqrt{1 - p^2}}$, si ponatur $\int \frac{dx}{X + \int \frac{dx}{X + \sqrt{X^2 - 1}}} + C = k$ et N basi logarithmorum hyperbolicorum, erit $\sqrt{1 - p^2} = \frac{N^{k\sqrt{-1}} - N^{-k\sqrt{-1}}}{2\sqrt{-1}}$ et $p = \frac{N^{k\sqrt{-1}} + N^{-k\sqrt{-1}}}{2}$, $\sqrt{1 - p^2}$ et p igitur sunt functiones ipsius x , quæ si ponantur $\sqrt{1 - X^2}$ et X , habetur $y (= \int \frac{pdx}{\sqrt{1 - p^2}}) = \int \frac{Xdx}{\sqrt{1 - X^2}}$, æquatio qua curvæ internoscuntur.

Si fit $F = Y$ functioni quantitatis y erit per Cor. 2. $\frac{dy}{Y} (= \frac{dy}{F}) = -\frac{dp}{1 - p^2}$ et integratione $\int \frac{dy}{Y} + \log. C = \log. \sqrt{1 - p^2}$, ponatur $\int \frac{dy}{Y} = k$ et N logarithmorum basi, erit facto ad quantitates absolutas transitu $CN^k = \sqrt{1 - p^2}$, $p = \sqrt{1 - C^2 N^{2k}}$ et $x (= \int \frac{dy \sqrt{1 - p^2}}{p}) = \int \frac{CN^k dy}{\sqrt{1 - C^2 N^{2k}}}$, æquatio quæ indolem curvæ indigat.

Si $F = Z$ functioni ipsius z erit $\frac{dz}{Z} (= \frac{dz}{F}) = -\frac{dp}{1 - p^2}$ et integratione $\int \frac{dz}{Z} + \log. C = \log. \sqrt{1 - p^2}$, et si $\int \frac{dz}{Z} = k$ et N basi logarithmica habetur $p = \frac{1 - C^2 N^{2k}}{1 + C^2 N^{2k}}$ et $y = \int p dz = \int \frac{1 - C^2 N^{2k} z}{1 + C^2 N^{2k}}$ qua curvæ cognoscuntur.

Constat hinc quod quoties $X + \int \dot{X} dx$ perfecta integratione habeatur $\int \frac{dx}{X + \int X + X dx}$ per arcus circulares dum $\frac{\int \dot{X} dx}{\sqrt{1 - \dot{X}^2}}$ absolutam admittat integrationem curva fit algebraica, si vero aliter evenierit transcendens.

Quoties $\frac{dy}{Y}$ fit integrale logarithmicum et $\frac{CN^k dy}{\sqrt{1 - C^2 N^{2k}}}$ absolutam admittat integrationem curva est algebraica, in aliis casibus transcendens.

Et quoties $\int \frac{dz}{Z}$ per logarithmos inveniatur, $\frac{1 - C^2 N^{2k} dz}{1 + C^2 N^{2k}}$ absolute fit integrabilis pariter ac $\frac{2CN^k dz}{1 + C^2 N^{2k}}$ curva est algebraica, alias transcendens.

Exempl. 1. Si fit variatio curvaturæ $T = \frac{2 \cdot b^2 - a^2 x \sqrt{a^2 - x^2}}{a^3 b}$ erit $\int T dx (= \frac{a^2 - 2 \cdot a^2 - x^2 \sqrt{a^2 - x^2}}{a^3 b}) = \frac{a \sqrt{a^2 - x^2}}{ab} - \frac{x^2 \sqrt{a^2 - x^2}}{ab} - \frac{b \sqrt{a^2 - x^2}}{a} + \frac{bx^2 \sqrt{a^2 - x^2}}{a^3}$, si ponatur $y = \frac{b \sqrt{a^2 - x^2}}{a}$ habetur $y + \int T dx = \frac{a^2 + b^2 - a^2 x^2 \sqrt{a^2 - x^2}}{a^3 b}$, adhibendo theorema $\frac{a^3 b dx}{a^4 + b^2 - a^2 x^2 \sqrt{a^2 - x^2}} (= \frac{dx}{y + \int T dx}) = - \frac{dp}{\sqrt{1 - p^2}}$ et integrando $\int \frac{a^3 b dx}{a^4 + b^2 - a^2 x^2 \sqrt{a^2 - x^2}} + C = - \int \frac{dp}{\sqrt{1 - p^2}}$, cujus termini sunt arcus circulares quorum sinus $\sqrt{1 - p^2} = \frac{a \sqrt{a^2 - x^2}}{\sqrt{a^4 + b^2 - a^2 x^2}}$ et cosinus $p = \frac{bx}{\sqrt{a^4 + b^2 - a^2 x^2}}$ evanescente arcu constanti C, quare $y (= \int \frac{p dx}{\sqrt{1 - p^2}}) = \int \frac{b dx}{a \sqrt{a^2 - x^2}} = \frac{b \sqrt{a^2 - x^2}}{a}$ et in hoc casu curva est ellipsis.

Exempl. 2. Sit jam variatio curvaturæ $T = \frac{2\sqrt{2ax+x^2}}{a}$ erit
 $\int Tdx = \frac{x\sqrt{2ax+x^2}}{a} + \int \frac{x dx}{\sqrt{2ax+x^2}}$ et posita $y = \int \frac{adx}{\sqrt{2ax+x^2}}$ per-
 fecta integratione habetur $y + \int Tdx = \frac{a+x\sqrt{2ax+x^2}}{a}$. Theore-
 matis itaque auxilio erit $\frac{adx}{a+x\sqrt{2ax+x^2}} (= \frac{dx}{y + \int Tdx} = -\frac{dp}{\sqrt{1-p^2}}$, et
 integratione $\int \frac{adx}{a+x\sqrt{2ax+x^2}} = C = -\int \frac{dp}{\sqrt{1-p^2}}$, si vero arcus ille
 constans $C = 0$ cæteri sunt æquales eorumque sinus et cosinus,
 unde $\sqrt{1-p^2} = \frac{\sqrt{2ax+x^2}}{a+x}$, $p = \frac{a}{a+x}$ et $y (= \int \frac{pdx}{\sqrt{1-p^2}}) = \int \frac{adx}{\sqrt{2ax+x^2}}$,
 æquatio indicans curvam esse catenariam.

THEOREMA III.

Dicatur cosinus anguli BCD q , posito radio 1, cæterisque
 manentibus denominationibus erit $\frac{dy}{\int Tdy-x} = \frac{dq}{\sqrt{1-q^2}}$.

Est enim $\frac{ay}{R} = aq$, qua per $\sqrt{1-q^2}$ divisa, dat $\frac{dy}{R\sqrt{1-q^2}} = \frac{dq}{\sqrt{1-q^2}}$;
 et ob $1 : \sqrt{1-q^2} :: CD (R) : CG = R\sqrt{1-q^2}$, fed $dx : dy ::$
 $Tdx : Tdy$ cujus integrale est $AE = \int Tdy$, unde $CG (=$
 $AE - AB) = \int Tdy - x$, qua pro $R\sqrt{1-q^2}$ substituta, prodit
 $\frac{dy}{\int Tdy-x} = \frac{dq}{\sqrt{1-q^2}}$.

Cor. 1. Semper $Tdy - dx$ admittit perfectam integrationem.
 Etenim $Tdy = \frac{ddy\sqrt{1-q^2}}{dq}$ et $dx = \frac{qdy}{\sqrt{1-q^2}}$, quibus $Tdy - dx =$
 $\frac{ddy\sqrt{1-q^2}}{dq} - \frac{qdy}{\sqrt{1-q^2}}$ et integratione $\int Tdy - x = \frac{dy\sqrt{1-q^2}}{aq}$.

R r r 2

Cor.

Cor. 2. Dicatur femichorda curvaturæ CG G, habetur
 $\frac{dy}{G} = \frac{dq}{\sqrt{1-q^2}}, \frac{dx}{G} = \frac{q dq}{1-q^2} \text{ et } \frac{dz}{G} = \frac{dq}{1-q^2}.$

Cor. 3. Dicatur cotangens anguli BCD t , et cofecans v erit
 $\frac{dy}{\int T dy - x} = \frac{dt}{1+t^2} \text{ et } \frac{dy}{\int T dy - x} = \frac{dv}{v \sqrt{v^2-1}}.$

Schol. 1. Quoniam $\int T dy - x = \frac{dy \sqrt{1-q^2}}{dq}$ datur ex data relatione
inter y et q , fit $\frac{dy \sqrt{1-q^2}}{dq} = Y$ functioni ordinatæ y erit $\int T dy =$
 $Y - x$ sumtisque fluxionibus $T dy = \dot{Y} dy - dx$ qua $T = \dot{Y} - \frac{dx}{dy}$
functioni ipsius y . Si autem $\frac{dy \sqrt{1-q^2}}{dq} = Q$ functioni ipsius q
erit $\int T dy = Q - x$ et sumtis fluxionibus $T dy = \dot{Q} dq - dx$, qua
habetur $T = \frac{\dot{Q} dq}{dy} - \frac{q}{\sqrt{1-q^2}}$ per q .

Schol. 2. Hujus theorematis auxilio elicere licet curvas data
relatione inter T et y , G et y , G et x , G et z , et G et q .
Si enim fit T functio ipsius y generaliter $\int T dy = Y + \int \dot{Y} dy + A$,
quæ functio est algebraica ipsius y quoties $\int \dot{Y} dy$ absolute sumi
possit. Assumatur $x = \int \dot{Y} dy$, tali ipsius y functioni ut non
solum $\int T dy - x = Y + \int \dot{Y} + \dot{Y} dy$ sed etiam $\int T dz = \int T dy$
 $\sqrt{1 - \dot{Y}^2}$ absoluta integratione habeantur, provenit vi theo-
rematis $\frac{dy}{Y + \int \dot{Y} + \dot{Y} dy + A} (= \frac{dy}{\int T dy - x}) = \frac{dq}{\sqrt{1-q^2}}$ et integratione
 $\int \frac{dy}{Y + \int \dot{Y} + \dot{Y} dy + A} + C = \int \frac{dq}{\sqrt{1-q^2}}.$ Posita $\frac{dy}{Y + \int \dot{Y} + \dot{Y} dy + A}$
 $+ C = I$ et N basi logarithmica erit $q = \frac{N^{I\sqrt{-1}} - N^{-I\sqrt{-1}}}{2\sqrt{-1}}$ et $\sqrt{1-q^2}.$

$= \frac{N^{1/2} - 1 + N^{-1/2} - 1}{2}$ quæ functiones sunt quantitatis y , quibus
positis Y et $\sqrt{1 - Y^2}$ prodit $x (= \int \frac{ady}{\sqrt{1 - q^2}}) = \int \frac{Y^{1/2}}{\sqrt{1 - Y^2}}$ æqua-
tio quæ indolem curvarum indicat.

Si $G = X$ functioni ipsius x erit per Cor. 2. $\frac{dx}{X} (= \frac{dx}{G}) = \frac{qdq}{1 - q^2}$,
et integratione $\log. CN^l (= \int \frac{dx}{X} + \log. C) = \log. \frac{1}{\sqrt{1 - q^2}}$ si $\int \frac{dx}{X}$
 $= l$, exinde $\sqrt{1 - q^2} = \frac{1}{CN^l}$, $q = \frac{\sqrt{C^2 N^{2l} - 1}}{CN^l}$ et $y (= \int \frac{dx \sqrt{1 - q^2}}{q})$
 $= \int \frac{dx}{\sqrt{C^2 N^{2l} - 1}}$, quæ curvæ naturam indigitat.

Si $G = Z$ functioni ipsius z erit $\frac{dz}{Z} (= \frac{dz}{G}) = \frac{dq}{1 - q^2}$, et integra-
tione $\log. CN^l (= \frac{dz}{Z} + C) = \log. \sqrt{\frac{1 + q}{1 - q}}$ si $\int \frac{dz}{Z} = l$, unde $q =$
 $\frac{C^2 N^{2l}}{1 + C^2 N^{2l}} \sqrt{1 - q^2} = \frac{2CN^l}{1 + C^2 N^{2l}} x (= \int q dz) = \int \frac{C^2 N^{2l} - 1 dz}{1 + C^2 N^{2l}}$ et $y (=$
 $\int dz \sqrt{-1 q^2}) = \int \frac{2CN^l dz}{1 + C^2 N^{2l}}$ quibus curvæ cognoscuntur.

Patet hinc quod quando $Y + \int Y dy$ algebraice habeatur
 $\int \frac{dy}{Y + \int Y dy + A}$ per quadraturam circuli, et $\int \frac{Y^{1/2} dy}{\sqrt{1 - Y^2}}$ etiam
obtineatur algebraice, curvæ evadunt algebraicæ, secus vero
transcendentes.

Quando $\int \frac{dx}{X}$ vel $\int \frac{dz}{Z}$ obtineatur per logarithmos, et
 $\int \frac{dx}{\sqrt{C^2 N^{2l} - 1}}$, vel tam $\int \frac{C^2 N^{2l} - 1 dz}{1 + C^2 N^{2l}}$ quam $\int \frac{2CN^l dz}{1 + C^2 N^{2l}}$ absoluta in-
tegratione, curvæ erunt algebraicæ.

Exempl.

Exempl. 1. Sit index variationis curvaturæ $T = \frac{6y}{a}$ erit $\int T dy = \frac{3y^2}{a} + A$, si quantitas illa constans $A = \frac{a}{2}$ quod evenit quum $\int T dy = \frac{a}{2}$ et $y = 0$; fumatur $x = \frac{y^2}{a}$ erit vi theorematis $\frac{2ady}{a^2 + 4y^2}$ $(= \frac{dy}{T dy - x}) = \frac{dq}{\sqrt{1-q^2}}$ et integratione $\int \frac{2ady}{a^2 + 4y^2} + C = \int \frac{dq}{\sqrt{1-q^2}}$, cujus æquationis termini quoniam sint arcus circulares quorum finus $q = \frac{2y}{\sqrt{a^2 + 4y^2}}$ et cofinus $\sqrt{1-q^2} = \frac{a}{\sqrt{a^2 + 4y^2}}$, arcu constanti $C = 0$, obtinetur $x (= \int \frac{q dy}{\sqrt{1-q^2}}) = \frac{y^2}{a}$ æquatio pro parabola Apolloniana.

Exempl. 2. Si sit $T = \frac{a^2}{y \sqrt{a^2 - y^2}}$ habetur $\int T dy = \int \frac{dy \sqrt{a^2 - y^2}}{y} - \sqrt{a^2 - y^2} + A$, si quantitas illa constans $A = 0$ quod evenit quum $\int T dy = 0$ et $y = a$, et assumatur $x = \int \frac{dy \sqrt{a^2 - y^2}}{y}$, evadit per theorema $-\frac{dy}{\sqrt{a^2 - y^2}} (= \frac{dy}{T dy - x}) = \frac{dq}{\sqrt{1-q^2}}$, et per integrationem $-\int \frac{dy}{\sqrt{a^2 - y^2}} + C = \int \frac{dq}{\sqrt{1-q^2}}$, quorum arcuum finus $q = \frac{\sqrt{a^2 - y^2}}{a}$ et cofinus $\sqrt{1-q^2} = \frac{y}{a}$ si constans ille $C = 0$, atque inde $dx \left(\frac{q dy}{\sqrt{1-q^2}} \right) = \frac{dy \sqrt{1-y^2}}{y}$ qua patet curvam esse tractoriam.

THEOREMA IV.

Dicatur summa tangentium angulorum HCD et BCD H, et differentia tangentium angulorum HCD et CKB K, retentis reliquis denominationibus erit $\frac{dx}{\int H dx} = -\frac{dp}{\sqrt{1-p^2}}$ et $\frac{dy}{\int K dy} = \frac{dq}{\sqrt{1-q^2}}$.
Quoniam

Quoniam $dy = \frac{p dx}{\sqrt{1-p^2}}$ erit $dy + T dx = T + \frac{p}{\sqrt{1-p^2}} dx$ et quum $H = T + \frac{p}{\sqrt{1-p^2}}$ habetur $dy + T dx = H dx$. Eodem modo quum $dx = \frac{q dy}{\sqrt{1-q^2}}$ erit $\int T dy - dx = T - \frac{q}{\sqrt{1-q^2}} dy$, sed $K = T - \frac{q}{\sqrt{1-q^2}}$, unde $\int T dy - x = K dy$. Per theorema igitur 2 et 3 provenit $\frac{dx}{\int H dx} = -\frac{dp}{\sqrt{1-p^2}}$ et $\frac{dy}{\int K dy} = \frac{dq}{\sqrt{1-q^2}}$.

Cor. Si fit ut antea tangens anguli BCD r , cotangens t , fecans s , et cofecans v , erit $\frac{dx}{\int H dx} = -\frac{dr}{r+r^2}$ et $\frac{dx}{\int H dx} = -\frac{ds}{s\sqrt{s^2-1}}$, $\frac{dy}{\int K dy} = \frac{dt}{1+t^2}$ et $\frac{dy}{\int K dy} = \frac{dv}{v\sqrt{v^2-1}}$.

Schol. Ope hujus theorematism invenire licet curvas, data relatione inter H et x atque K et y . Itaque fit $H = X$ functioni ipsius x erit $\int H dx = \int X dx + A$, vi theorematism $\frac{dx}{\int X dx + A} (= \frac{dx}{\int H dx}) = -\frac{dp}{\sqrt{1-p^2}}$, et integratione $\int \frac{dx}{\int X dx + A} + C = -\int \frac{dp}{\sqrt{1-p^2}}$. Posita $\int \frac{dx}{\int X dx + A} + C = m$, et N logarithmorum basi prodit $\sqrt{1-p^2} = \frac{N^m \sqrt{-1} - N^{-m} \sqrt{-1}}{2\sqrt{-1}}$ et $p = \frac{N^m \sqrt{-1} + N^{-m} \sqrt{-1}}{2}$, quibus functionibus quantitatis x positim $\sqrt{1-X^2}$ et X provenit æquatio $y (= \int \frac{p dx}{\sqrt{1-p^2}}) = \frac{\int X dx}{\sqrt{1-X^2}}$ naturam curvarum exprimens.

Si $K = Y$ functioni quantitatis y , eadem calculandi ratione habetur $x (= \int \frac{q dy}{\sqrt{1-q^2}}) = \frac{\int Y dy}{\sqrt{1-Y^2}}$ æquatio qua curvæ cognoscuntur.

Quando

Quando $\int Xdx$ vel $\int Ydy$ absoluta integratione, $\int \frac{dx}{\sqrt{Xdx+A}}$ vel $\int \frac{dy}{\sqrt{Ydy+A}}$ per rectificationem circuli, et $\int \frac{Xdx}{\sqrt{1-X^2}}$ vel $\int \frac{Ydy}{\sqrt{1-Y^2}}$ integratione perfecta obtineantur, curva est algebraica.

Exempl. 1. Si fit $H = \frac{a+12x}{2\sqrt{a}\sqrt{x}}$ erit $\int Hdx = \frac{a+4x\sqrt{x}}{\sqrt{a}} + A$, et posita $A=0$ habetur per theorema $\frac{dx\sqrt{a}}{a+4x\sqrt{x}} (= \frac{dx}{\int Hdx}) = -\frac{dp}{\sqrt{1-p^2}}$ et per integrationem $\int \frac{dx\sqrt{a}}{a+4x\sqrt{x}} + C = \int \frac{dp}{\sqrt{1-p^2}}$, cujus termini quum sint arcus circulares quorum sinus $\sqrt{1-p^2} = \frac{2\sqrt{x}}{\sqrt{a+4x}}$ et cofinus $p = \frac{\sqrt{a}}{\sqrt{a+4x}}$, posita $C=0$, obtinetur $y (= \int \frac{pdx}{\sqrt{1-p^2}}) = \sqrt{ax}$, quæ parabolam Apolloniam exprimit.

Exempl. 2. Sit $H = \frac{2a^4-x^4}{ax^2\sqrt{a^2-x^2}}$ erit $\int Hdx = \frac{x^2-2a^2\sqrt{a^2-x^2}}{ax} + A$, et si $A=0$, per theorema $\frac{axdx}{x^2+2a^2\sqrt{a^2-x^2}} (= \frac{dx}{\int Hdx}) = -\frac{dp}{\sqrt{1-p^2}}$ et per integrationem $\int \frac{axdx}{x^2+2a^2\sqrt{a^2-x^2}} + C = -\int \frac{dp}{\sqrt{1-p^2}}$, et si $C=0$, $\sqrt{1-p^2} = \frac{\sqrt{a^2-x^2}}{\sqrt{2a^2-x^2}}$ $p = \frac{a}{\sqrt{2a^2-x^2}}$ et $y (= \int \frac{pdx}{\sqrt{1-p^2}}) = \int \frac{adx}{\sqrt{a^2-x^2}}$ æquatio pro curva sinuum.

Exempl. 3. Si fit $K = \frac{5a^2+6y^2 \cdot y}{a\sqrt{a^2+y^2}}$ erit $\int Kdy = \frac{a^2+2y^2\sqrt{a^2+y^2}}{a^2} + A$, si $A=0$ habetur per theorema $\frac{a^2dy}{a^2+2y^2\sqrt{a^2+y^2}} (= \frac{dy}{\int Kdy}) = \frac{dq}{\sqrt{1-q^2}}$ et integratione $\int \frac{a^2dy}{a^2+2y^2\sqrt{a^2+y^2}} + C = -\int \frac{dq}{\sqrt{1-q^2}}$, qua $q = \frac{y}{\sqrt{a^2+y^2}}$, $\sqrt{1-q^2} = \frac{\sqrt{a^2+y^2}}{\sqrt{a^2+y^2}}$, si $C=0$, unde $x (= \int \frac{qdy}{\sqrt{1-q^2}}) = \sqrt{a^2+y^2}$ æquatio pro hyperbola æquilatera.

Exempl.

Exempl. 4. Sit $K = \frac{y}{\sqrt{a^2 - y^2}}$ erit $\int K dy = A - \sqrt{a^2 - y^2}$ et si $A = 0$, per theorema $-\frac{dy}{\sqrt{a^2 - y^2}} (= \frac{dy}{\int K dy}) = \frac{dq}{\sqrt{1 - q^2}}$ et per integrationem $-\int \frac{dy}{\sqrt{a^2 - y^2}} + C = \int \frac{dq}{\sqrt{1 - q^2}}$ qua $q = \frac{\sqrt{a^2 - y^2}}{a}$, $\sqrt{1 - q^2} = \frac{y}{a}$ et $dx (= \frac{q dy}{\sqrt{1 - q^2}}) = \frac{dy \sqrt{a^2 - y^2}}{y}$ quæ Tractoriam exprimit.

THEOREMA V.

Designetur productum tangentium angulorum HCD et BCD per U, et angulorum HCD et CKB per V cæteris manentibus erit $\frac{dx}{\int U dx - x} = -\frac{dp}{p}$ et $\frac{dy}{y + \int V dy} = \frac{dq}{q}$.

Quoniam $dy = \frac{p dx}{\sqrt{1 - p^2}}$ et $U = \frac{T p}{\sqrt{1 - p^2}}$ erit $T dy (= \frac{T p dx}{\sqrt{1 - p^2}}) = U dx$, et integratione $\int T dy = \int U dx$ qua $\int T dy - x = \int U dx - x$. Et quoniam $dx = \frac{q dy}{\sqrt{1 - q^2}}$ et $V = \frac{T q}{\sqrt{1 - q^2}}$ erit $T dx (= \frac{T q dy}{\sqrt{1 - q^2}}) = V dy$, $\int T dx = \int V dy$ et $y + \int T dx = y + \int V dy$. Theoremate 2. et 3. prodit $\frac{dx}{\int U dx - x} = -\frac{dp}{p}$ et $\frac{dy}{y + \int V dy} = \frac{dq}{q}$.

Cor. Si anguli BCD tangens, cotangens, &c. designentur ut antea, habetur $\frac{dx}{\int U dx - x} = -\frac{dr}{r \cdot 1 + r^2}$, $\frac{dy}{y + \int V dy} = -\frac{dt}{t \cdot 1 + t^2}$, &c.

Schol. Per hoc theorema curvæ inveniuntur ex data relatione inter U et x, atque inter V et y. Si enim fit $U = X$ functioni ipsius x erit $\int U dx = \int X dx + A$, per theorema $\frac{dx}{\int X dx - x + A} (= \frac{dx}{\int U dx - x}) = -\frac{dp}{p}$, et per integrationem $\int \frac{dx}{\int X dx - x + A} + \log. C =$

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log.

log. $\frac{1}{p}$. Ponatur $\int \frac{dx}{Xdx-x+A} = z$ et N basi logarithmica, erit

$$\frac{1}{p} = CN^n, \quad p = \frac{1}{CN^n}, \quad \sqrt{1-p^2} = \frac{\sqrt{C^2N^{2n}-1}}{CN^n} \quad \text{et } y \left(= \frac{pdx}{\sqrt{1-p^2}} \right) =$$

$\int \frac{dx}{\sqrt{C^2N^{2n}-1}}$ qua æquatione curvarum indoles innotescit.

Si $V = Y$ functioni ipsius y , eadem calculandi ratione proveni
t $x \left(= \int \frac{qdy}{\sqrt{1-q^2}} \right) = \int \frac{CN^n dy}{\sqrt{1-C^2N^{2n}}}$ qua curvæ cognoscuntur.

Evidens hinc est quod quoties $\int Xdx$ vel $\int Ydy$ algebraice
 $\int \frac{dx}{Xdx-x+A}$ vel $\int \frac{dy}{y+\int Ydy+A}$ per logarithmos, atque $\int \frac{dx}{\sqrt{C^2N^{2n}-1}}$
vel $\int \frac{CN^n dy}{\sqrt{1-C^2N^{2n}}}$ integratione absoluta, obtineantur, curva est
algebraica.

Exempl. 1. Si fit $U = 3$ erit $\int Udx = 3x + A$, si vero $\int Udx =$
 $\frac{a}{2}$ quando $x = 0$ erit $A = \frac{a}{2}$ et $\int Udx - x = \frac{a+4x}{2}$. Per theorema
igitur $\frac{2dx}{a+4x} \left(= \frac{dx}{\int Udx-x} \right) = -\frac{dp}{p}$ et per integrationem log.
 $\sqrt{a+4x} + \log. C = \log. \frac{1}{p}$, posita $p = 1$ dum $x = 0$ log. $C = -$
log. \sqrt{a} , unde facto a logarithmis transitu $\frac{\sqrt{a+4x}}{\sqrt{a}} = \frac{1}{p}$, qua $p =$
 $\frac{\sqrt{a}}{\sqrt{a+4x}}$, $\sqrt{1-p^2} = \frac{2\sqrt{x}}{\sqrt{a+4x}}$ et $y \left(= \int \frac{pdx}{\sqrt{1-p^2}} \right) = \int \frac{dx\sqrt{a}}{2\sqrt{x}} = \sqrt{ax}$
æquatio pro Parabola Apolloniana.

Exempl. 2. Sit $U = \frac{x^3-4a^3}{x\sqrt{x}}$ erit $\int Udx = \frac{x^3-2a^3}{3x^2} + A$, si autem
 $\int Udx = 0$ et $x = a^3/2$, erit $A = 0$ et $\int Udx - x = \frac{2 \cdot a^3 - x^3}{3x^2}$. Vi
igitur theorematibus erit $\frac{3x^2 dx}{2a^3 - x^3} \left(= \frac{dx}{\int Udx-x} \right) = -\frac{dp}{p}$, et integratione

$\log. \frac{a\sqrt{a}}{\sqrt{a^3-x^3}} + \log. C = \log. \frac{1}{p}$ qua $p = \frac{\sqrt{a^3-x^3}}{a\sqrt{a}}$; $\sqrt{1-p^2} = \frac{x\sqrt{x}}{a\sqrt{a}}$

et $y (= \int \frac{pdx}{\sqrt{1-p^2}}) = \frac{dx \sqrt{a^3-x^3}}{x\sqrt{x}}$ æquatio ad curvam quæsitam.

Exempl. 3. Si $V = -\frac{1}{2}$ erit $\int Vdy = A - \frac{y}{2}$, posita $\int Vdy = 0$ et $y = 0$ erit $A = 0$ et $y + \int Vdy = \frac{y}{2}$. Per theorema obtinetur $\frac{2dy}{y} (= \frac{dy}{y + \int Vdy}) = \frac{dq}{q}$ et per integrationem $\log. y^2 + \log. C = \log. q$, si $q = 1$ et $y = a$ erit $\log. C = -\log. a^2$, unde $q = \frac{y^2}{a^2}$, $\sqrt{1-q^2} = \frac{\sqrt{a^4-y^4}}{a^2}$ atque $dx (= \frac{qdy}{\sqrt{1-q^2}}) = \frac{y^2dy}{\sqrt{a^4-y^4}}$, curva ergo est Elastica,

Exempl. 4. Sit $V = \frac{a^2-2y^2}{y^2}$ erit $\int Vdy = A - \frac{a^2+2y^2}{y}$, si $\int Vdy = -3a$ et $y = a$ erit $A = 0$, indeque $y + \int Vdy = -\frac{a^2+y^2}{y}$. Theorematis ope habetur $-\frac{ydy}{a^2+y^2} (= \frac{dy}{y + \int Vdy}) = \frac{dq}{q}$ et integratione $\log. \frac{1}{\sqrt{a^2+y^2}} + \log. C = \log. q$, si $q = 1$ et $y = 0$ erit $\log. C = \log. a$ et exinde $q = \frac{a}{\sqrt{a^2+y^2}}$, $\sqrt{1-q^2} = \frac{y}{\sqrt{a^2+y^2}}$ et $dx (= \frac{qdy}{\sqrt{1-q^2}}) = \frac{ady}{y}$ æquatio pro Logarithmica.

THEOREMA VI.

Dicatur ED L, et AE M, retentis præterea adhibitis denominationibus erit $\frac{dL}{T} = dx$ et $\frac{dM}{T} = dy$.

Quoniam $dz : dx :: Tdz (dR) : Tdx$ habetur $dL = Tdx$ et
S f f 2 dL

$\frac{dL}{T} = dx$. Et quoniam $dx : dy :: Tdx (dR) : Tdy$ obtinetur $dM = Tdy$ et $\frac{dM}{T} = dy$.

Cor. Quum $Tdy = Udx$ et $Tdx = Vdy$, erit substitutione $\frac{dM}{U} = dx$ et $\frac{dL}{V} = dy$.

Schol. Hoc adhibito theoremate inveniri possunt curvæ data relatione inter T et L , T et M , atque inter U et M et V et L . Ponatur $L = T$ functioni quantitatis T habetur per theorema $\frac{dT}{T} (= \frac{dL}{T}) = dx$ et integratione $\int \frac{dT}{T} + C = x$ qua T per x datur. Curvæ deinde per theorema 2. elici possunt.

Si $M = T$ ipsius T functioni, habetur eodem modo T per y . Si $M = U$ functioni ipsius U , obtinetur U per x , et si $L = V$ functioni quantitatis V , datur V per y . Per theorema deinde 3. et 5. curvæ inveniuntur.

Evidens quidem est quod curvæ esse non possunt algebraicæ nisi $\int \frac{dL}{T}$, $\int \frac{dM}{T}$, $\int \frac{dM}{U}$ vel $\int \frac{dL}{V}$, obtineantur integratione absoluta.

Exempl. 1. Si fit $L = \frac{aT^3}{54}$ erit $dL = \frac{aT^2 dT}{18}$, et per hoc theorema $\frac{aTdT}{18} (= \frac{dL}{T}) = dx$ et integratione $\frac{aT^2}{36} + C = x$ qua $T = \frac{6\sqrt{x}}{\sqrt{a}}$, si $C = 0$. Per theorema 2. reperitur $y = \sqrt{ax}$, æquatio pro Parabola Apolloniana.

Exempl. 2. Si fit $M = - \int \frac{aT^2 dT}{2 \cdot 1 + T^2}$ erit $dM = - \frac{aT^2 dT}{2 \cdot 1 + T^2}$ et ope theorematis $-\frac{aTdT}{2 \cdot 1 + T^2} (= \frac{dM}{T}) = dy$, et integratione $\frac{a}{4 \cdot 1 + T^2} + C = y$, qua si $C = 0$, $T = \frac{\sqrt{a-4y}}{2\sqrt{y}}$. Per theorema 3. habetur

$$dx =$$

$dx = \frac{2dy\sqrt{y}}{\sqrt{a-4y}}$, æquatio pro Cycloide ordinaria.

Exempl. 3. Sit $L = -a\sqrt{V}$ erit $dL = -\frac{adV}{2\sqrt{V}}$ et per theorema $-\frac{adV}{2\sqrt{V}} (= \frac{dL}{V}) = dy$ et integratione $\frac{a}{\sqrt{V}} + C = y$, et si $C = 0$, habetur $V = \frac{a^2}{y^2}$ et deinde per theorema 5. $dx = \frac{dy\sqrt{a^2-y^2}}{y}$, qua constat curvam esse Tractoriam.

THEOREMA VII.

Dicatur ut antea CF F et CG G, et summa tangentium angularum HCD et BCD, H, et differentia tangentium angularum HCD et CKB, K, erit $\frac{dF}{H} = dx$ et $\frac{dG}{K} = dy$.

Quoniam $dF (= dy + Tdx) = Hdx$ et $dG (= \int Tdy - x) = Kdy$ provenit $\frac{dF}{H} = dx$ et $\frac{dG}{K} = dy$.

Cor. Quum $F = -\frac{dx\sqrt{1-p^2}}{dp}$ et $G = \frac{dy\sqrt{1-q^2}}{dq}$ provenit divisione $\frac{dF}{FH} = -\frac{dp}{\sqrt{1-p^2}}$ atque $\frac{dG}{GK} = \frac{dq}{\sqrt{1-q^2}}$.

Schol. Auxilio hujus theorematis inveniuntur curvæ ex data relatione inter F et H, G et K, H et p atque K et q. Nam si fit $F = H$ functioni ipsius H, vel $G = K$ functioni ipsius K, habetur per theorema $\frac{dH}{H} (= \frac{dF}{H}) = dx$ et integratione $\int \frac{dH}{H} + C = x$ quæ H per x datur. Eodem modo $\frac{dK}{K} (= \frac{dG}{K}) = dy$ et integratione $\int \frac{dK}{K} + C = y$ qua K per y obtinetur. Theorema 4. ulterius progredienti viam monstrat ad curvas inveniendas.

Patet

Patet quod curva non fit algebraica nisi $\int \frac{dH}{H}$ vel $\int \frac{dK}{K}$ obtineantur perfecta integratione.

Exempl. 1. Si fit $F = \frac{a}{\sqrt{1+H^2}}$ habetur per theorema $-\frac{adH}{1-H^2} (= \frac{dF}{H}) = dx$, et integratione $\frac{aH}{\sqrt{1-H^2}} + C = -x$ qua $H = -\frac{x}{\sqrt{a^2-x^2}}$, posita $C=0$. Per theorema deinde 4. provenit $y = \sqrt{a^2-x^2}$ æquatio pro circulo.

Exempl. 2. Sit $F = \frac{a \cdot H^3 + H^3 + 6\sqrt{H^2-12}}{108}$, erit per theorema $\frac{a \cdot H^2 - 6 + H\sqrt{H^2-12} \cdot dH}{36\sqrt{H^2-12}} (= \frac{dF}{H}) = dx$ et integratione facta $\frac{a \cdot H^2 - 6 + H\sqrt{H^2-12}}{72} + C = x$, et posita $C=0$ habetur $H = \frac{a+12x}{2\sqrt{a}\sqrt{x}}$, unde per theorema 4. prodit $y = \sqrt{ax}$ æquatio pro Parabola Apolloniana.

Exempl. 3. Sit $G = -\frac{a \cdot 4+K^2}{4}$ erit per theorema $\frac{adK}{2} (= \frac{dG}{K}) = dy$, et integratione $\frac{aK}{2} + C = y$, et si $C=0$ $K = \frac{2y}{a}$ unde per theorema 4. $dx = \frac{ady}{y}$, qua constat curvam esse Logarithmicam.

THEOREMA VIII.

Dicatur ut antea productum tangentium angulorum HCD et BCD U, et productum tangentium angulorum HCD et CKB V manentibus reliquis denominationibus erit $\frac{dG}{U-1} = dx$ et $\frac{dF}{1+V} = dy$.

Quoniam

Quoniam $G = \int Tdy - x$ erit $dG = Tdy - dx$, sed $Tdy = Udx$, unde $dG = \overline{U-1}dx$ et $\frac{dG}{U-1} = dx$. Eodem modo quum $F = y + \int Tdx$ erit $dF = dy + Tdx$, sed $Tdx = Vdy$ quare $dF = \overline{1+V}dy$ et $\frac{dF}{1+V} = dy$.

Cor. Quoniam $G = \frac{dy\sqrt{1-q^2}}{dq}$ et $F = -\frac{dx\sqrt{1-p^2}}{dp}$, habetur substitutione debita $\frac{dG}{G \cdot U-1} = -\frac{dp}{p}$ et $\frac{dF}{F^2 \cdot 1+V} = \frac{dq}{q}$.

Schol. Ope hujus theorematis indagantur curvæ data relatione inter G et U vel inter F et V . Nam si sit $G = \dot{U}$ functioni quantitatis U vel $F = \dot{V}$ functioni quantitatis V obtinetur per theorema in casu priori $\frac{d\dot{U}}{U-1} (= \frac{dG}{U-1}) = dx$ et integratione

$\int \frac{d\dot{U}}{U-1} + C = x$, qua U per x habetur; in posteriori $\frac{d\dot{V}}{1+V} (= \frac{dF}{1+V}) = dy$ et integratione $\int \frac{d\dot{V}}{1+V} + C = y$, qua V habetur per y . Per theorema deinde 5. curvæ cognoscuntur.

Datur etiam per Cor. U in p , et V in q , et consequenter T in p vel q , nam $U = \frac{Tp}{\sqrt{1-p^2}}$ et $V = \frac{Tq}{\sqrt{1-q^2}}$.

Constat hinc quod curvæ non sint algebraicæ nisi $\int \frac{d\dot{U}}{U-1}$ vel $\int \frac{d\dot{V}}{1+V}$ obtineantur integratione absoluta.

Exempl. 1. Si sit $G = \frac{a \cdot \overline{U-3}}{2}$ erit per theorema $\frac{a d\dot{U}}{2 \cdot \overline{U-1}} (= \frac{dG}{U-1}) = dx$ et integratione $\log. 1 - U + \log. C = \frac{2x}{a}$ et si $C = \frac{a^2}{2}$ log.

$\log. \frac{a^2 \cdot \overline{1-U}}{2} = \frac{2x}{a}$ et $\frac{a \cdot \overline{1-U}}{2} = N^{\frac{2x}{a}}$ qua $U = \frac{a^2 - 2N^{\frac{2x}{a}}}{a^2}$. Per theorema deinde 5. habetur $dy = \frac{dx N^{\frac{x}{a}}}{a}$ qua constat curvam esse Logarithmicam.

Exempl. 2. Si sit $T = \frac{a \cdot \overline{V-1}\sqrt{V+2}}{3\sqrt{3}}$ erit per theorema $\frac{adV}{2\sqrt{3}\sqrt{V+2}} (= \frac{dF}{1+V}) = dy$ et per integrationem $\frac{a\sqrt{V+2}}{\sqrt{3}} = y$ qua $V = \frac{3y^2 - 2a^2}{a^2}$; et per theorema 5. $dx = \frac{dy \sqrt{y^2 - a^2}}{a}$, æquatio ad curvam cujus constructio a quadratura hyperbolæ dependet.

THEOREMA IX.

Sint LC et lc duæ curvæ eandem habentes Evolutam QD, dicatur radorum osculi CD cD constans differentia cC b , curvæ lc variatio curvaturæ S, ceterisque ut antea manentibus erit

$$\frac{dR}{R-bS} = - \frac{dp}{\sqrt{1-p^2}}.$$

Quoniam radius curvaturæ DH evolutæ sit $RT = R - bS$, erit $\frac{1}{R-bS} \doteq \frac{1}{RT}$, quæ per $dR (= Tdz) = - \frac{RTdp}{\sqrt{1-p^2}}$ multiplicata, monstrat esse $\frac{dR}{R-bS} = - \frac{dp}{\sqrt{1-p^2}}.$

Cor. Si sint ut antea tangens anguli BCD r et secans s , habetur $\frac{dR}{R-bS} = - \frac{dr}{1+r^2}$ et $\frac{dR}{R-bS} = - \frac{ds}{s\sqrt{s^2-1}}.$

Schol. Subsidio hujus theorematis invenire licet curvas, data relatione inter S et R vel inter S et T nam $\frac{S}{T} = \frac{R}{R-b}$. Itaque si ponatur

ponatur $S = R$ functioni radii curvædinis R , erit $\frac{dR}{R-bR} (= \frac{dR}{R-bS})$
 $= -\frac{dp}{\sqrt{1-p^2}}$, et integratione $\int \frac{dR}{R-bR} + C = - \int \frac{dp}{\sqrt{1-p^2}}$. Sit
 $\int \frac{dR}{R-bR} + C = f$ et N logarithmorum basi habetur $\sqrt{1-p^2} =$
 $\frac{N^{\sqrt{1-p^2}} - N^{-\sqrt{1-p^2}}}{2\sqrt{1-p^2}}$ et $p = \frac{N^{\sqrt{1-p^2}} + N^{-\sqrt{1-p^2}}}{2}$ functionibus quantitatis
 R , quibus R per p exprimi potest. Per theorema igitur 1.
 curvas internoscere valemus.

Si $R = S$ functioni quantitatis S habetur $\frac{dS}{S-bS} (= \frac{dR}{R-bS})$
 $= -\frac{dp}{\sqrt{1-p^2}}$, et integratione $\int \frac{dS}{S-bS} + C = - \int \frac{dp}{\sqrt{1-p^2}}$, posita
 $\int \frac{dS}{S-bS} + C = g$, erit $\sqrt{1-p^2} = \frac{N^g \sqrt{1-p^2} - N^{-g} \sqrt{1-p^2}}{2 - \sqrt{1-p^2}}$ et $p = \frac{N^g \sqrt{1-p^2} + N^{-g} \sqrt{1-p^2}}{2}$
 quibus S per p datur. Per theoremata Partis I. invenire licet
 curvas omnes eandem evolutam habentes.

Hinc videtur, quod curvæ non sint algebraicæ nisi $\int \frac{dR}{R-bR}$
 vel $\int \frac{dS}{S-bS}$ per circuli rectificationem obtineatur.

Exempl. 1. Si fit $S = \frac{2R}{\sqrt{a} \cdot \sqrt{R-a}}$ supposita $b=a$, erit per
 theorema $\frac{dR \sqrt{a}}{2R \sqrt{R-a}} (= \frac{dR}{R-bS}) = -\frac{dp}{\sqrt{1-p^2}}$ et integratione
 $\int \frac{dR \sqrt{a}}{2R \sqrt{R-a}} + C = - \int \frac{dp}{\sqrt{1-p^2}}$, si vero arcus ille constans $C=0$
 erit $\sqrt{1-p^2} = \frac{\sqrt{R-a}}{\sqrt{R}}$ qua $R = ap^2$, et per Cor. 1. Theor. 1. ha-
 betur $dy = \frac{adx}{\sqrt{x^2 - a^2}}$, æquatio pro Catenaria.

Exempl. 2. Sit $S = \frac{5a^2 + R^2}{a \cdot a - 5R}$, posita $b = \frac{a}{5}$ erit per theorema
 $-\frac{adR}{a^2 + R^2} (= \frac{dR}{R - bS}) = -\frac{dp}{\sqrt{1-p^2}}$ et facta integratione $-\int \frac{adR}{a^2 + R^2} + C$
 $= -\int \frac{dp}{\sqrt{1-p^2}}$, qua si $C=0$, habetur $\sqrt{1-p^2} = \frac{R}{\sqrt{a^2 + R^2}}$ et $R =$
 $\frac{\sqrt{1-p^2}}{p}$. Per theorema 1. $dx = \frac{dy\sqrt{a^2 - y^2}}{y}$ qua constat curvam
 esse Tractoriam.

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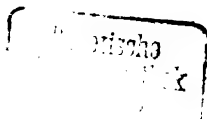
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V O L. LXXIII.

Page. Line.

478. 18. *for* brighter *read* less bright

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289. 8. *for* the *read* those
290. 11. *dele* rather
291. 8. *for* the *read* these
329. 7. *for* 1784 *read* 1783
341. 23. *for* , first, *read* fixed
342. 12. *for* than *read* that
345. 1. *after* from *insert* their
365. 11. *for* I *read* K

In plate XIV. fig. 2. the letter *a* should be inserted at the bottom, and the letter *b* at the top, of the dotted *interval* $19\frac{1}{2}$, in the same manner as the letters *c* and *d* at the two extremes of the dotted *intervals* in fig. 3.

In plate XXI. fig. 2. at the lower angle of the diagram, in some copies, the letter K stands by mistake instead of H.

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